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CONJUGATE SYSTEMS OF SPACE CURVES WITH EQUAL LAPLACE-DARBOUX INVARIANTS

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It is the object of this note to provide a new geometrical interpretation for the condition that the Laplace-Darboux invariants of an equation of the form

$$\frac{\partial^2 z}{\partial x \partial y} + a\frac{\partial z}{\partial x} + b\frac{\partial z}{\partial y} + cz = 0 \tag{1}$$

be equal. These invariants are

$$h = a_x + ab - c, \quad k = b_y + ab - c, \tag{2}$$

where we have put

$$a_x = \frac{\partial a}{\partial x}, \quad b_y = \frac{\partial b}{\partial y}, \tag{3}$$

a notation which we shall employ throughout this paper.

Let $z^{(1)}$, $z^{(2)}$, $z^{(3)}$, $z^{(4)}$ be any four linearly independent solutions of (1), functionally independent in the sense that no two of their three ratios can be expressed as a function of the third. If $z^{(1)}, \ldots, z^{(4)}$ be interpreted as the homogeneous coordinates of a point P of space, variation of the parameters x and y will cause P to generate a non-degenerate surface S which is not a plane. Let us assume further that S is not developable. Then $z^{(1)}, \ldots, z^{(4)}$ will satisfy, besides (1), just one other linear homogeneous differential equation of the second order, of the form

$$z_{yy} = mz_{xx} + nz_x + pz_y + qz, \tag{4}$$

where

$$m = \frac{D''}{D}, \quad D \not\equiv 0, \quad D'' \not\equiv 0, \tag{5}$$

D and D'' being two of the fundamental quantities of S of the second order. The third of these quantities, D', is equal to zero on account of the fact that the curves $x = \text{const.}$ and $y = \text{const.}$ form a conjugate system on S.

Let us now apply the Laplace transformations to the surface S, by putting

$$z_1 = z_y + az, \quad z_{-1} = z_x + bz. \tag{6}$$

These expressions determine two points P_1 and P_{-1} whose loci give rise to two further surfaces S_1 and S_{-1}. The surface S_1 is the second sheet of the focal surface of the congruence formed by the lines which are tangent to the curves $x = \text{const.}$ on S; the surface S_{-1} is connected in similar fashion with the curves $y = \text{const.}$ on S.

Consider now the line which joins the points P_1 and P_{-1} of the 1st and -1st Laplace transformed nets. There is one such line for every point P of the original surface S. Consequently, the totality of these lines forms a *congruence*, whose developables we proceed to determine. For this purpose, let us give increments, δx and δy, to x and y. The coordinates of those points of S_1 and S_{-1}, which correspond to the point $z + z_x\,\delta x + z_y\delta y$ of S, will be

$$Z_1 = z_1 + \frac{\partial z_1}{\partial x}\delta x + \frac{\partial z_1}{\partial y}\delta y, \qquad Z_{-1} = z_{-1} + \frac{\partial z_{-1}}{\partial x}\delta x + \frac{\partial z_{-1}}{\partial y}\delta y.$$

Now we find, making use of (1),

$$\begin{aligned} z_1 &= z_y + az, & z_{-1} &= z_x + bz, \\ \frac{\partial z_1}{\partial x} &= -bz_y + (a_x - c)\,z, & \frac{\partial z_{-1}}{\partial x} &= z_{xx} + bz_x + b_x z, \\ \frac{\partial z_1}{\partial y} &= z_{yy} + az_y + a_y z, & \frac{\partial z_{-1}}{\partial y} &= -az_x + (b_y - c)\,z. \end{aligned} \tag{7}$$

The homogenous coordinates of an arbitrary point on the line $Z_1 Z_{-1}$ will be obtained from λZ_1 and μZ_{-1}, and this expression, on account of (7), assumes the form

$$\begin{aligned} &\{\lambda\,[a + (a_x - c)\,\delta x + (a_y + q)\,\delta y] + \mu\,[b + b_x\delta x + (b_y - c)\,\delta y]\}\,z \\ &+ \{\lambda n\delta y + \mu\,(1 + b\delta x - a\delta y)\}\,z_x + \lambda\,\{1 - b\delta x + (a + p)\,\delta y\}\,z_y \\ &+ (\mu\delta x + \lambda m\delta y)\,z_{xx}. \end{aligned} \tag{8}$$

In order that such a point may also be on the line $P_1 P_{-1}$, i.e., in order that these two lines may intersect, (8) must reduce to a linear homo-

geneous combination of z_1 and z_{-1}. This will be so, if and only if λ, μ, δx, and δy can be determined subject to the two conditions

$$\lambda m\delta y + \mu\delta x = 0, \tag{9}$$

$$\lambda\left[h + (a_y + q - bn - a^2 - ap)\,\delta y\right] + \mu\left[(b_x - b^2)\,\delta x + k\delta y\right] = 0.$$

If we eliminate the ratio $\lambda : \mu$ from (9), we obtain the differential equation of the developables of the congruence, viz.:

$$h\delta x^2 + \left[a_y + q - bn - a^2 - ap - m\,(b_x - b^2)\right]\delta x\delta y - mk\delta y^2 = 0. \tag{10}$$

This equation may also be regarded as the differential equation of the curves which the developables of the congruence determine on S. The asymptotic curves of S are determined by

$$D\delta x^2 + D''\delta y^2 = 0. \tag{11}$$

Therefore the curves (10) form a conjugate system on S if and only if the simultaneous invariant of (10) and (11) is equal to zero, i.e., if and only if $hD'' - mkD = 0$. But, according to (5), this reduces to $h = k$.

We have, therefore, obtained the following theorem. *Consider a net of conjugate curves on a non-developable surface S. Let P be any point of this net and let P_1 and P_{-1} be the corresponding points of the nets obtained from the given one by the 1st and -1st Laplace transformations. Consider further the congruence of all of the lines such as $P_1\,P_{-1}$. The curves on S, which correspond to the developables of this congruence, will form a conjugate system, if and only if the original net of conjugate curves has equal Laplace-Darboux invariants.*

I wish to add one further remark. Darboux[1] has given a geometric interpretation of the condition $h = k$, different from mine, by means of a certain conic in the plane of P, P_1, P_{-1}.

I have found it advisable to introduce two such conics, which coincide with each other and with that of Darboux in the case $h = k$ and only in that case. By using these conics I have been able, quite recently, to interpret geometrically the condition which Bianchi expresses by saying that a conjugate system is *isothermally conjugate*. These systems have made their appearance in so many problems of differential geometry that such a geometrical interpretation seems to me to be a matter of very great interest. I shall, however, reserve the details of this interpretation for a place, in its appropriate setting, in a paper on the general theory of congruences which I hope to present for publication before long.

[1] *Leçons sur la Théorie générale des Surfaces*, vol. 4, p. 38.

TRANSFORMATION OF SURFACES Ω

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When any surface S is referred to a conjugate system with equal point invariants, its cartesian coördinates x, y, z, are solutions of an equation of the form

$$\frac{\partial^2\theta}{\partial u\partial v}+\frac{\partial \log\sqrt{\rho}}{\partial v}\frac{\partial\theta}{\partial u}+\frac{\partial \log\sqrt{\rho}}{\partial u}\frac{\partial\theta}{\partial v}=0. \qquad (1)$$

If θ_i is any solution of this equation, linearly independent of x, y and z, the surface S_i whose cartesian coördinates, x_i, y_i, z_i, are given by equations of the form

$$\frac{\partial}{\partial u}(\lambda_i x_i)=-\rho\left(x\frac{\partial\theta_i}{\partial u}-\theta_i\frac{\partial x}{\partial u}\right),\quad \frac{\partial}{\partial v}(\lambda_i x_i)=\rho\left(x\frac{\partial\theta_i}{\partial v}-\theta_i\frac{\partial x}{\partial v}\right), \qquad (2)$$

where λ_i is given by the quadratures

$$\frac{\partial\lambda_i}{\partial u}=-\rho\frac{\partial\theta_i}{\partial u},\quad \frac{\partial\lambda_i}{\partial v}=\rho\frac{\partial\theta_i}{\partial v}, \qquad (3)$$

is referred to a conjugate system with equal point invariants, and corresponding points M and M_i on S and S_i are harmonic with respect to the focal points of the line MM_i for the congruence of these lines. We say that S_i is obtained from S by a *transformation K*. We have studied these transformations at length in a recent memoir.[1] In the present note we consider the case where the lines MM_i form a normal congruence. In this case there exists a solution t of equation (1) such that $x^2+y^2+z^2-t^2$ also is a solution. Thus the parametric conjugate system is 2 O in the sense of Guichard, and t is the *complementary function*. The surface S_i has the same properties.

By definition a *surface C* is one possessing a conjugate system 2 O with equal point invariants. When this system is parametric, the first fundamental coefficients of C have the form

$$E=\left(\frac{\partial t}{\partial u}\right)^2+\frac{1}{\rho},\quad F=\frac{\partial t}{\partial u}\frac{\partial t}{\partial v},\quad G=\left(\frac{\partial t}{\partial v}\right)^2+\frac{1}{\rho}, \qquad (4)$$

and this property is characteristic.

It can be shown that *when a surface C is referred to the system 2 O with equal point invariants, there can be found without quadratures two sur-*

faces of the same kind, say C_0 and C_0', such that the congruences of lines MM_0 and MM_0' are normal. Furthermore, the spheres of radius t with their centers on C are enveloped by two surfaces, each of which is orthogonal to one of the congruences. These orthogonal surfaces are *surfaces* Ω as defined by Demoulin,[2] who showed that they are characterized by the property that their fundamental coefficients $\bar{E}$, $\bar{G}$, $\bar{D}$, $\bar{D}''$, when the lines of curvature are parametric, satisfy the condition

$$\frac{\partial}{\partial u}\left(\frac{\frac{\partial}{\partial v}\left(\frac{\bar{D}''}{\bar{G}}\right)\sqrt{\bar{G}}\,\bar{U}}{\frac{\bar{D}}{\bar{E}}-\frac{\bar{D}''}{\bar{G}}\sqrt{\bar{E}}\,\bar{V}}\right)+\frac{\partial}{\partial v}\left(\frac{\frac{\partial}{\partial u}\left(\frac{\bar{D}}{\bar{E}}\right)\sqrt{\bar{E}}\,\bar{V}}{\frac{\bar{D}}{\bar{E}}-\frac{\bar{D}''}{\bar{G}}\sqrt{\bar{G}}\,\bar{U}}\right)=0, \tag{5}$$

where $\bar{U}$ and $\bar{V}$ are functions of u and v respectively.

If X, Y, Z; X_1, Y_1, Z_1; X_2, Y_2, Z_2 denote the direction-cosines of the normal to a surface C, and of the bisectors of the angles between the tangents to the parametric curves, we may write the equations of a transformation K in the form (cf. *Transactions*, loc. cit.)

$$x_i - x = \frac{1}{m_i\lambda_i}(a_iX_1+b_iX_2+\omega_iX), \tag{6}$$

where m_i is a constant and a_i, b_i, ω_i are functions satisfying the completely integrable system of equations

$$\begin{aligned}
\frac{\partial a_i}{\partial u} &= -m_i(\lambda_i-\rho\theta_i)\sqrt{E}\cos\omega + b_iA + \frac{w_iD}{2\sqrt{E}\cos\omega},\\
\frac{\partial a_i}{\partial v} &= -m_i(\lambda_i+\rho\theta_i)\sqrt{G}\cos\omega - b_iB + \frac{w_iD''}{2\sqrt{G}\cos\omega},\\
\frac{\partial b_i}{\partial u} &= m_i(\lambda_i-\rho\theta_i)\sqrt{E}\sin\omega - a_iA = \frac{w_iD}{2\sqrt{E}\sin\omega},\\
\frac{\partial b_i}{\partial v} &= -m_i(\lambda_i+\rho\theta_i)\sqrt{G}\sin\omega + a_iB + \frac{w_iD''}{2\sqrt{E}\sin\omega},\\
\frac{\partial w_i}{du} &= -\frac{D}{2\sqrt{E}}\left(\frac{a_i}{\cos\omega}-\frac{b_i}{\sin\omega}\right),\quad \frac{\partial w_i}{dv} = -\frac{D''}{2\sqrt{G}}\left(\frac{a_i}{\cos\omega}+\frac{b_i}{\sin\omega}\right),
\end{aligned} \tag{7}$$

where 2ω is the angle between the parametric lines on C, D and D'' are the second fundamental coefficients of C, and

$$A = \sqrt{\frac{E}{G}}\frac{\partial\log\sqrt{\rho}}{\partial v}\sin 2\omega - \frac{\partial\omega}{\partial u},\quad B = \sqrt{\frac{G}{E}}\frac{\partial\log\sqrt{\rho}}{\partial u}\sin 2\omega - \frac{\partial\omega}{\partial v}.$$

For the sake of brevity we put

$$T_i^2 = a_i^2 + b_i^2 + w_i^2, \quad H = \sqrt{EG - F^2}. \tag{8}$$

The functions a_0, b_0, w_0, λ_0, θ_0 which determine C_0 are given by

$$a_0 = -\sin\omega \cdot w_0\rho\left(\sqrt{G}\frac{\partial t}{\partial u} + \sqrt{E}\frac{\partial t}{\partial v}\right),$$

$$b_0 = \cos\omega \cdot w_0\rho\left(\sqrt{G}\frac{\partial t}{\partial u} - \sqrt{E}\frac{\partial t}{\partial v}\right),$$

$$\frac{\partial \log w_0}{\partial u} = \frac{D}{H}\frac{\partial t}{\partial u}, \quad \frac{\partial \log w_0}{\partial v} = \frac{D''}{H}\frac{\partial t}{\partial v}, \quad T_0 = H\rho w_0, \tag{9}$$

$$\frac{m_0}{T_0}(\lambda_0 - \rho\theta_0) = \frac{D}{H} + \frac{1}{\dfrac{\partial t}{\partial u}}\frac{\partial}{\partial u}\log H\rho, \quad \frac{m_0}{T_0}(\lambda_0 + \rho\theta_0) = \frac{D''}{H} + \frac{1}{\dfrac{\partial t}{\partial v}}\frac{\partial}{\partial v}\log H\rho.$$

The complementary function t_0 for C_0 is given by

$$t_0 = t - \frac{T_0}{\lambda_0 m_0}. \tag{10}$$

The functions for the surface C_0' are analogous to the above.

Ordinarily the surfaces S_i derived from a surface C by transformations K are not surfaces C. However, the equations

$$\begin{aligned}
&\frac{\partial\theta_1}{\partial u} + \sqrt{E}\left[\cos\omega a_1 - \sin\omega b_1 + (t_1 - t)\frac{m_1\lambda_1}{T_0}(\cos\omega a_0 - \sin\omega b_0)\right] \\
&\frac{\partial\theta_1}{\partial v} + \sqrt{G}\left[\cos\omega a_1 + \sin\omega b_1 + (t_1 - t)\frac{m_1\lambda_1}{T_1}(\cos\omega a_0 + \sin\omega b_0)\right]
\end{aligned} \tag{11}$$

are consistent with equations (7) for $i = 1$, the function θ_1 so defined is a solution of equation (1), and the new surface S_1, given by (6), is a surface C, say C_1. In particular we remark that the function t_1 given by (2), when x_i and x are replaced by t_1 and t respectively, is the complementary function for C_1. Furthermore, if x_i and x in equations (2) are replaced by $x_1^2 + y_1^2 + z_1^2 - t_1^2$ and $x^2 + y^2 + z^2 - t_1^2$ the resulting equations are satisfied.

With the aid of the theorem of permutability of general transformations K (cf. *Transactions*, loc. cit., p. 406) we show that *if C, C_0 and C_1 are three surfaces in the relation indicated above, a fourth surface C_{10} can be found without quadratures such that the lines joining corresponding points M_1, M_{10}, on C_1 and C_{10} is a normal congruence.*

Likewise it is found that *the surfaces Ω and Ω_1 normal to the congruences of the lines MM_0 and M_1M_{10} at the distances t and t_1 from C and C_1 respectively envelope a two-parameter family of spheres, and the lines of*

curvature on Ω *and* Ω_1 *correspond.* Thus equations (7) and (11) define transformations of surfaces Ω of the Ribaucour type. We call them *transformations A*. When in particular the surfaces C and C_1 are *associate* surfaces, which is a special case of transformations K, the surfaces C_0 and C_{10} are likewise associate, and the surfaces Ω and Ω_1, as defined in the preceding theorem, have the same spherical representation of their lines of curvature.

By means of a generalized theorem of permutability for transformations K in general we prove the following theorem of permutability for transformations A: *If* Ω_1 *and* Ω_2 *are two surfaces obtained from a surface* Ω *by transformations A, there exists a surface* Ω' *which is in the relations of transformations A with* Ω_1 *and* Ω_2, *and* Ω' *can be found without quadratures.*

Isothermic surfaces are surfaces C for which $t = 0$. In this case the transformations A are equivalent to the transformations D_m of isothermic surfaces, discovered by Darboux and studied at length by Bianchi.[3]

Surfaces with isothermal representation of their lines of curvature are surfaces Ω in the sense that the surface C is the locus of the point midway between the centers of principal curvature of Ω, and C_0 is at infinity. This case requires special study, but the results are analogous to those of the general case. However, the transformations A are now the same as the transformations of these surfaces established from another point of view by me.[4]

[1] Eisenhart, *Trans. Amer. Math. Soc.* **15**, 397–430 (1914).
[2] Demoulin, *Paris, C. R. Acad. Sci.*, **153**, 703 (1911).
[3] Bianchi, *Annali Mat. Milano*, Ser. 3, **11**, 93–158 (1905).
[4] Eisenhart, *Trans. Amer. Math. Soc.*, **9**, 149–177 (1908).

POTASSIUM AMMONO ARGENATE, BARATE, CALCIATE, AND SODATE

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It has been shown by me that reactions strictly analogous to those which accompany the solution of the hydroxides of zinc, lead, and aluminium in aqueous solutions of potassium hydroxide take place when the amides of certain metals are treated with liquid-ammonia solutions of potassium amide. Thus, just as zinc hydroxide is known to dissolve in an aqueous solution of potassium hydroxide to form potassium aquo-

zincate, so potassium amide in liquid ammonia reacts with zinc amide to form potassium ammonozincate.[1] These analogous reactions are represented by the equations:

$$Zn(OH)_2 + 2KOH = Zn(OK)_2 + H_2O,$$
$$Zn(NH_2)_2 + 2KNH_2 = Zn(NHK)_2 + 2NH_3.$$

In view of the many close analogies which have been shown to exist between the derivatives of water and the derivatives of ammonia the formation of this potassium ammonozincate and of potassium ammonostannate[2] and potassium ammonoplumbite[3] was not unexpected. Further work has, however, shown that elements which do not give rise to similar salts in water form these compounds in ammonia. Thus in previous articles this was shown to be true of copper[4] and thallium;[5] and researches have now been completed which show it to be true of silver, cadmium, and nickel. The work on silver will be here briefly described; that on cadmium and nickel, which was carried out by G. S. Bohart, will be later reported upon.

Some years ago while studying the action of potassium amide on liquid-ammonia solutions of silver salts[6] it was observed that silver amide dissolves readily in an excess of potassium amide solution. This solubility recalled the amphoteric properties of the hydroxides of aluminium, zinc, and lead, and suggested that in all probability the solution resulting from the action of a solution of potassium amide on silver amide would be found to contain an ammonoargentate of potassium.

It was found in fact that a crop of beautiful crystals of the composition represented by the formula $AgNHK.NH_3$ or $AgNH_2.KNH_2$ is obtained when a liquid-ammonia solution prepared by dissolving silver amide in an excess of potassium amide is first adjusted to a proper concentration and then cooled in a bath of liquid ammonia. After two or three recrystallizations the salt is pure. The equation expressing its formation is $AgNH_2 + KNH_2 = AgNHK + NH_3$. Because of the relationship of the compound to the familiar (aquo) zincates and aluminates it is appropriately called potassium ammonoargentate.

More surprising was the result of a later investigation[7] in which the amide of such a strongly electropositive element as magnesium was found to react with potassium amide to form potassium ammonomagnesate, a compound of the formula $Mg(NHK)_2 \cdot 2NH_3$. This made it seem probable that potassium amide possesses the property of forming similar compounds with the amides of all the metals. It was therefore deemed advisable to study the action of solutions of potassium amide on the

amides of the strongly electropositive barium, strontium, and calcium, and on the amide of sodium, in order to determine whether potassium ammonobarate, ammonostrontiate, and ammonocalciate, and possibly even potassium ammonosodate, can be prepared.

When a barium salt in solution in liquid ammonia is added to a solution of potassium amide a microcrystalline precipitate is obtained which has been found to have the composition represented by the formula, $BaNK.2NH_3$.

When solutions of strontium and calcium salts in liquid ammonia are poured into an excess of a solution of potassium amide there are formed apparently noncrystalline products, which are represented respectively by the formulas, $SrNK.2NH_3$ and $CaNK.2NH_3$. The calciate was also prepared by the action of potassium amide on metallic calcium.

When sodium amide is treated with a liquid-ammonia solution of potassium amide, or when a solution of a soluble salt of sodium is added to an excess of potassium amide in solution in liquid ammonia beautiful crystals of a product whose composition is represented by the formula $NaNK_2.2NH_3$, separate from the solution.

For the reason that these compounds have been prepared by methods entirely similar to those whereby potassium ammonomagnesate and the other salts of the same class mentioned above are formed they have received the names given in the title of this paper and are formulated as salts with ammonia of crystallization. It is possible, however, to formulate them as molecular compounds of potassium amide with the respective amides of the less positive metals, as follows, $Ba(NH_2)_2.KNH_2$, $Sr(NH_2)_2.KNH_2$, $Ca(NH_2)_2.2NH_3$, $NaNH_2.2NHK_2$; thus representing them as ammonia analogues of Werner's hydroxo salts.

It is hoped that transference measurements which are to be undertaken in this laboratory may show for such of these compounds as are sufficiently soluble whether or not the less positive metal travels together with the nitrogen toward the anode during electrolysis.

[1] Fitzgerald, *J. Amer. Chem. Soc.*, **29**, 657; Franklin, Ibid., **29**, 1274 (1907).
[2] Fitzgerald, *J. Amer. Chem. Soc.*, **29**, 1693 (1907).
[3] Franklin, *J. Phys. Chem.*, **15**, 509 (1911).
[4] Franklin, *J. Amer. Chem. Soc.*, **34**, 1501 (1912).
[5] Franklin, *J. Phys. Chem.*, **16**, 682 (1912).
[6] Franklin, *J. Amer. Chem. Soc.*, **27**, 835 (1905).
[7] Franklin, *J. Amer. Chem. Soc.*, **34**, 1455 (1913).

METALLIC SALTS OF AMMONO ACIDS

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The Action of Liquid-Ammonia Solutions of Ammono Acids on Metallic Amides, Imides, and Nitrides. The acid amides and imides, and the metallic derivatives of the acid amides and imides are the acids, bases, and salts respectively of an ammonia system of acids, bases, and salts.[1]

Guided by the relationships implied in the above statement Franklin and Stafford were able to prepare potassium derivatives of a considerable number of acid amides by the action of potassium amide on certain acid amides in solution in liquid ammonia. That is to say, an ammono base, potassium amide, was found to react with ammono acids in liquid ammonia to form ammono salts just as the aquo base, potassium hydroxide, acts upon aquo acids in water solution to form aquo salts. Choosing, for example, benzamide and benzoic acid as representative acids of the two systems, the analogous reactions taking place respectively in liquid ammonia and water are represented by the equations:

$$C_6H_5CONH_2 + KNH_2 = C_6H_5CONHK + NH_3.$$
$$C_6H_5CONH_2 + 2KNH_2 = C_6H_5CONK_2 + 2NH_3.$$
$$C_6H_5COOH + KOH = C_6H_5COOK + H_2O.$$

The ammono acid, since it is dibasic, reacts with either one or two molecules of potassium amide to form an acid and a neutral salt.

Having thus demonstrated the possibility of preparing ammono salts of potassium by the interaction of potassium amide and acid amides in liquid ammonia solution, it was further found that ammono salts of the heavy metals may be prepared by the action of liquid ammonia solutions of ammono acids on insoluble metallic amides, imides, and nitrides—that is, by reactions which are analogous to the formation of aquo salts in water by the action of potassium hydroxide on insoluble metallic hydroxides and oxides.

In the present investigation the action of acetamide, benzenesulfonamide, and paratoluenesulfonamide on the amide of silver, on the imides of copper and lead, and on the nitride of thallium has been studied.

The method for the preparation of a typical compound may be briefly described. Pure silver amide, prepared in a two-legged reaction-tube[2] by thoroughly washing the precipitate resulting from the interaction of potassium amide and silver nitrate in liquid-ammonia solution, is dis-

solved in a liquid-ammonia solution of benzenesulfonamide. After adjusting the concentration of the solution and cooling in a bath of liquid ammonia a crop of beautiful crystals of the composition represented by the formula, $C_6H_5SO_2NHAg.2NH_3$, separates from the solution. On standing in a vacuum at 20° the salt loses one molecule of ammonia of crystallization. Heated at 200° it loses the second molecule.

The action of the acid on the base is shown by the equation:

$$C_6H_5SO_2NH_2 + AgNH_2 = C_6H_5SO_2NHAg + NH_3.$$

In a similar way the following salts have been obtained:

Thallous ammonobenzenesulfonate, $C_6H_5SO_2NHTl$, $C_6H_5SO_2NHTl.NH_3$, and $.2NH_3$.
Cuprous ammonobenzenesulfonate, $C_6H_5SO_2NHCu$ and $C_6H_5SO_2NHCu.2NH_3$.
Thallous ammonoparatoluenesulfonate, $CH_3C_6H_4SO_2NHTl$, and $CH_3C_6H_4SO_2NHTl.2NH_3$.
Cuprous ammonoparatoluenesulfonate, $CH_3C_6H_4SO_2NHCu$, and $CH_3C_6H_4SO_2NHCu.2NH_3$.
Silver ammonoacetate, $CH_3CONHAg$ and $CH_3CONHAg.2NH_3$.
Thallous ammonoacetate, $CH_3CONHTl$ and $CH_3CONHTl.2NH_3$.

The Action of Liquid-Ammonia Solutions of Ammono Acids on Metallic Hydroxides and Oxides. Reactions between some of the stronger ammono acids and metallic hydroxides are known to take place in water solutions. In order to ascertain whether metallic hydroxides and oxides would dissolve in liquid ammonia solutions of acid amides to form ammono salts, cupric hydroxide and silver oxide were submitted to the action of liquid-ammonia solutions of benzenesulfonamide with the result that reactions took place as represented by the equations:

$$Cu(OH)_2 + 2C_6H_5SO_2NH_2 = 2C_6H_5SO_2NHCu + 2H_2O$$
$$Ag_2O + C_6H_5SO_2NH_2 = C_6H_5SO_2NHAg + H_2O,$$

Dried *in vacuo* at 20° cupric ammonobenzenesulfonate retains four molecules of ammonia of crystallization. The silver salt is identical with the silver ammonobenzenesulfonate described above.

The Action of Liquid-Ammonia Solutions of Acid Ammono Esters on Ammono Bases. Such compounds as acetanilide and other aryl and alkyl derivatives of the acid amides are acid ammono esters which by virtue of their acid properties should be capable of reacting in liquid-ammonia solutions with metallic amides to form compounds which are at the same time ammono esters and ammono salts.

Four such compounds have been prepared; namely, silver phenyl ammonoacetate, $CH_3CONAgC_6H_5$ and $CH_3CONAgC_6H_5.NH_3$, by the action of a solution of acetanilide on silver amide; thallous phenyl ammonoacetate, $CH_3CONTlC_6H_5$, by the action of acetanilide on thallous nitride; potassium benzyl ammonoacetate, $CH_3CONKCH_2C_6H_5.NH_3$,

by the interaction of benzylacetamide and potassium amide; and potassium phenetol ammonoacetate, $CH_3CONKC_6H_4OC_2H_5$, by the interaction of phenetolacetamide and potassium amide.

The nature of these reactions will be clear from the equation,

$$(C_6H_5)(CH_3CO)NH + AgNH_2 = (C_6H_5)(CH_3CO)NAg + NH_3,$$

which represents the action of acetanilide on silver amide, or in other words, the action of an acid ammono ester on an ammono base.

[1] Franklin, *Amer. Chem. J.*, **47**, 285 (1912).
[2] Franklin, *J. Amer. Chem. Soc.*, **27**, 820 (1905).

AMMONOBASIC IODIDES OF ALUMINIUM

By Edward C. Franklin

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While investigating the action of potassium amide on salts of aluminium in liquid-ammonia solution it was noted that a considerable amount of potassium amide could be added to aluminium iodide solutions without the formation of a permanent precipitate. Under analogous conditions in aqueous solutions aquobasic salts are formed.

The attempt was therefore made to isolate an ammonobasic aluminium iodide from a liquid-ammonia solution of aluminium iodide to which potassium amide had been added in an amount not quite sufficient to produce a permanent precipitate. When a solution so prepared is carefully concentrated a crop of well formed crystals of a compound is obtained which, after several recrystallizations, gave analytical results sharply in accord with the formula, $AlI_3 . Al(NH_2)_3 . 6NH_3$. The equation expressing its formation is $2AlI_3 + 3KNH_2 = AlI_3 . Al(NH_2)_3 + 3KI$.

The compound is obviously related to ammonia as the ordinary basic salts are related to water. It is therefore an ammonobasic salt.

When sufficient potassium amide is added to a liquid-ammonia solution of aluminium iodide to produce a permanent precipitate a second ammonobasic salt is formed the composition of which is represented by the formula $Al(NH_2)_3 . Al(NH_2)_2I . NH_3$.

A REVISION OF THE ATOMIC WEIGHT OF LEAD: THE ANALYSIS OF LEAD BROMIDE AND CHLORIDE

By Gregory Paul Baxter, Fred Leslie Grover and Thorbergur Thorvaldson

COOLIDGE MEMORIAL LABORATORY, HARVARD UNIVERSITY

Presented to the Academy, January 3, 1914

In a recent paper by Baxter and Wilson[1] the atomic weight of lead was found from analyses of fused lead chloride, by comparison with pure silver and by weighing the silver chloride produced. The result of this investigation, Pb = 207.09 (Ag = 107.880), is considerably higher than most earlier determinations. For this reason and because the work of Baxter and Wilson was essentially preliminary in its nature it was obviously desirable to test the matter further, and ever since the publication of the above paper the atomic weight of lead has been under almost continuous investigation in the Harvard Laboratory. Because of the advantages offered by the greater insolubility of silver bromide, the analysis of lead bromide was next undertaken; and later lead chloride was investigated anew. The outcome of all the work is that the atomic weight of lead is very close to 207.20, a value even higher than that previously found by Baxter and Wilson.

The method of experimentation was the usual one employed with metallic halides. Carefully purified lead bromide, which had been crystallized from concentrated hydrobromic acid solution, was dried by fusion in an atmosphere containing a large proportion of hydrobromic acid gas. The acid was synthesized from hydrogen and bromine, and during a fusion was mixed with either nitrogen, hydrogen, or bromine. After being weighed the salt was dissolved in hot water containing a very small amount of nitric acid, and at low concentration the solution was precipitated with a very dilute solution of a carefully weighed, very nearly equivalent quantity of pure silver. The solution was allowed to stand for some days with occasional shaking, and then was tested in a nephelometer for excess of silver or bromide. If a deficiency of either was found, this deficiency was made up with very dilute standard silver or bromide solution. Then the solution was again tested, and the operations were repeated if necessary. As soon as the exact end-point had been found, a moderate excess of silver nitrate was added, and eventually the silver bromide was collected and weighed. The usual correction for moisture retained by the dried silver bromide was determined and applied.

In order to test the purity of the lead salt, metallic electrodes were prepared from the purified material, and the ultraviolet spark spectrum of the metal was photographed by means of a Féry quartz spectrograph. The only lines which could be detected were those commonly ascribed to lead. In fact the original material was fairly pure, containing only traces of silver, copper, and iron, which rapidly disappeared in the course of the purification. Incidentally we found that 0.004% of copper in lead yields fairly strong copper lines and that 0.0001% of silver could readily be detected. Our material was further tested in a sensitive gold leaf electroscope, but showed no evidence of radioactivity.

Several difficulties were met which deserve special attention. Lead bromide, when dissolved in water, hydrolyses to give an insoluble basic bromide. This phenomenon was first met with material which had been fused in an atmosphere containing a large proportion of hydrobromic acid gas, but it was also found that even unfused salt which has been crystallized from concentrated hydrobromic acid solution, and which probably contains a small amount of the latter substance, hydrolyses in the same way, and, apparently to about the same extent. Thus it was evident that at any rate the greater part of the hydrolysis occurred during the solution of the salt and not during the fusion. The evidence that *no* hydrolysis with loss of bromine occurred during the fusion is that the different samples of fused salt, in spite of the widely varied atmospheres in which they were fused, were essentially identical in composition and that the atomic weight of lead found from the bromide agrees very closely with that found later in this research by analysis of the chloride.

In order to prevent the basic salt from forming during the solution of the bromide, a small quantity of nitric acid was added to the hot water in which the salt was dissolved. It was proved experimentally that the nitric acid added was far too small in amount to cause loss of either bromine or hydrobromic acid from the solution.

A second difficulty was caused by the presence of a small amount of black insoluble material in the fused salt. Although puzzling at first, this was eventually found to consist partly of carbon, partly of silica. The greatest care in the purification of the material failed to eliminate the trouble entirely, although the proportion of insoluble residue was reduced on an average to 0.004%. Error from this source was avoided by filtering the lead bromide solution through a weighed platinum-sponge crucible in each analysis before precipitation of the silver bromide, and weighing the residue.

The weights given in the tables are converted to the vacuum standard by applying the following corrections for each apparent gram of substance: $PbBr_2$, +0.000036; $PbCl_2$, +0.000060. Ag, −0.000031; AgBr. +0.000041; AgCl, +0.000071.

The analyses in the following table were made by F. L. Grover. Two preliminary series, one by T. Thorvaldson and one by Grover, gave essentially the same result, but since the insoluble residue was not determined in these experiments, the details are not given.

THE ATOMIC WEIGHT OF LEAD

$PbBr_2 : 2Ag : 2AgBr$

Ag = 107.880 Br = 79.916

FUSION ATMOSPHERE	GRAMS OF $PbBr_2$	GRAMS OF Ag	RATIO $PbBr_2$:2Ag	ATOMIC WEIGHT OF Pb	GRAMS OF AgBr	RATIO $PbBr_2$: 2AgBr	ATOMIC WEIGHT OF Pb
$HBr+H_2$	5.27845	3.10271	1.70124	207.23			
$HBr+H_2$	2.65094	1.55822	1.70126	207.23			
$HBr+N_2$	4.08410	2.40104	1.70097	207.17			
$HBr+N_2$	4.97468	2.92473	1.70090	207.15			
$HBr+N_2$	4.05541	2.38398	1.70111	207.20	4.15017	0.97717[1]	207.18
$HBr+N_2$	3.44139	2.02288	1.70123	207.23	3.52224	0.97705	207.14
$HBr+N_2$	5.17387	3.04158	1.70105	207.19	5.29498	0.97713	207.17
$HBr+N_2$	3.84497	2.26022	1.70115	207.21	3.93446	0.97726	207.22
$HBr+H_2$	4.30513	2.53086	1.70105	207.19	4.40616	0.97707	207.15
$HBr+H_2$	4.53445	2.66549	1.70117	207.21	4.64048	0.97715	207.18
$HBr+N_2$	5.78437	3.40044	1.70107	207.19	5.91976	0.97713	207.17
$HBr+N_2$	4.87079	2.86337	1.70107	207.19	4.98467	0.97715	207.18
$HBr+N_2$	6.28446	3.69447	1.70104	207.19	6.43124	0.97718	207.19
$HBr+N_2$	4.74639	2.79011	1.70115	207.21	4.85708	0.97721	207.20
$HBr+N_2$	6.82424	4.01148	1.70118	207.21	6.98380	0.97715	207.18
$HBr+N_2$	6.53689	3.84274	1.70110	207.20			
$HBr+Br_2$	4.10098	2.41088	1.70103	207.18	4.19791	0.97691[1]	207.09[1]
$HBr+Br_2$	2.64256	1.55352	1.70101	207.18			
$HBr+Br_2$	6.30683	3.70718	1.70125	207.23			
		Average	1.70111	207.20		0.97715	207.18

[1] This result is omitted in computing the average.

Since the result of these analyses of lead bromide is even higher than that obtained by Baxter and Wilson, lead chloride also was investigated anew by essentially the same method as was used in the work with the bromide. The chloride was finally dried by fusion in a current of hydrochloric acid gas.

In preliminary experiments to our surprise a basic salt precipitated during the solution of the lead chloride in hot water, as soon as a very considerable proportion of the salt was dissolved, although a similar phenomenon was never observed in the earlier research upon the chloride.

One marked difference, however, exists between the two cases. In the experiments by Baxter and Wilson from 4 to 5 grams of salt were dissolved in a liter of water, whereas in our first experiments 8 to 9 grams of salt were dissolved in the same volume. In other words the basic salt precipitates only at high concentrations

As in the case of the bromide, uncertainty exists as to whether the basic salt is formed during the fusion of the slightly damp material, even in the presence of a high hydrochloric acid concentration, or whether it is due to hydrolysis during solution. Since, however, we found that lead chloride which has been crystallized from dilute hydrochloric acid solution, when dissolved in enough hot water to yield a solution nearly saturated at room temperature (about 10 g. per liter), yields a similar insoluble compound which dissolves readily in dilute acid and contains chlorine, the latter explanation seems the more probable one.

More direct evidence that the fused chloride is neutral was obtained as follows: Chloride which had been crystallized from very dilute hydrochloric acid solution was dried in a vacuum at room temperature. Weighed portions were then fused in a current of hydrochloric acid gas and the loss in weight due to water was found. The fused portions were compared with silver in the usual way, and at the same time other weighed portions of the vacuum-dried material were similarly analyzed. After correction for the moisture retained by the unfused material, both fused and unfused salt were found to possess the same composition.

Further evidence that the fused salt is free from basic chloride, is afforded by the agreement of the chloride analyses among themselves, as well as with the results of the bromide analyses.

In dissolving the fused salt for analysis the precipitation of the basic salt was prevented as with the bromide by adding a small amount of nitric acid to the hot water in which the chloride was dissolved. It was proved experimentally that no chlorine is lost in this way.

Furthermore, as with the bromide, the fused chloride usually contained a trace of black insoluble matter, in spite of the utmost care in the purification of the salt and in the preparation of the hydrochloric acid gas in which the salt was fused. Since, however, this residue was collected upon a weighed platinum-sponge crucible, and since, after it had been weighed, a correction was applied, no appreciable error from this source could have occurred. The average per cent of the black residue found in the chloride was essentially the same as that found in the bromide.

The source of the lead material used in the preparation of the lead bromide and a portion of the lead chloride is unknown to us. Both commercial metal and commercial lead nitrate were employed. Be-

cause the atomic weight of radioactive lead has recently been found to differ from that of ordinary lead[2] we also undertook the examination of inactive lead from widely different geographical sources. Lead nitrate was prepared from the various minerals, and after crystallization was converted to lead chloride. The latter in turn was several times crystallized from dilute hydrochloric acid solution, fused and analyzed by comparison with silver. Each specimen of lead nitrate was carefully examined spectroscopically as previously described, and not the slightest difference could be detected between the different specimens. Furthermore the radioactivity of all the minerals was tested in a sensitive gold-leaf electroscope and no one of them was found to increase the normal leak of the instrument by a measurable amount.

In the experiments with lead chloride no attempt was made to collect the silver chloride obtained in the comparisons with silver. Instead, in separate experiments with new portions of the chloride, silver chloride was precipitated with an excess of silver nitrate and the precipitate was collected and weighed.

The analyses in the following table were made by Grover.

THE ATOMIC WEIGHT OF LEAD

	$PbCl_2 : 2Ag$ Ag = 107.880				$PbCl_2 : 2AgCl$ Cl = 35.457		
SOURCE OF MATERIAL	GRAMS OF $PbCl_2$	GRAMS OF Ag	RATIO $PbCl_2$: 2Ag	ATOMIC WEIGHT OF Pb	GRAMS OF AgCl	RATIO $PbCl_2$: 2AgCl	ATOMIC WEIGHT OF Pb
Commercial Nitrate	5.63567	4.37200	1.28904	207.21			
	5.58730	4.33427	1.28910	207.22			
	6.86319	5.32402	1.28910	207.22			
Average.....			1.28908	207.22			
Joplin, Mo, U. S. A.[1]	4.70770	3.65223	1.28899	207.20			
	4.20222	3.25968	1.28915	207.23			
Average			1.28907	207.22			
Wallace, Id. U. S. A.[2]	7.04688	5.46691	1.28901	207.20			
	5.88935	4.56868	1.28907	207.22			
Average			1.28904	207.21			
Wallace, Id. U. S. A.[2]	6.96370				7.17754	0.97021	207.22
	6.89046				7.10231	0.97017	207.21
Average						0.97019	207.21
Tucson, Ariz., U. S. A.[3]	4.90083	3.80171	1.28911	207.22			
	5.79300	4.49404	1.28904	207.21			

[1] Galena; [2] Cerussite; [3] Vanadinite and Wulfenite.

THE ATOMIC WEIGHT OF LEAD—Continued

SOURCE OF MATERIAL	GRAMS OF $PbCl_2$	GRAMS OF Ag	RATIO $PbCl_2$: 2Ag	ATOMIC WEIGHT OF Pb	GRAMS OF AgCl	RATIO $PbCl_2$: 2AgCl	ATOMIC WEIGHT OF Pb
Average			1.28908	207.22			
Metalline Falls, Wash, U. S. A.[1]	5.43965	4.21992	1.28908	207.21			
	5.74504	4.45674	1.28907	207.22			
Average			1.28906	207.21			
Nassau, Germany[1]	6.57216	5.09849	1.28904	207.21			
	5.66330	4.39340	1.28905	207.21			
Average			1.28905	207.21			
Eifel Mts., Germany[2]	5.73434	4.44857	1.28903	207.21			
	4.17445	3.23862	1.28896	207.19			
Average			1.28900	207.20			
New South Wales[2]	6.25884	4.85584	1.28893	207.19			
	5.25882	4.07933	1.28914	207.23			
Average			1.28904	207.21			
New South Wales[2]	7.33227				7.55732	0.97022	207.22
	6.51699				6.71690	0.97024	207.23
Average						0.97023	207.23
Average of all experiments			1.28905	207.21		0.97021	207.22

[1] Galena; [2] Cerussite.

SUMMARY OF ATOMIC WEIGHT VALUES

$PbBr_2$: 2Ag	207.20
$PbBr_2$: 2AgBr	207.18
$PbCl_2$: Ag	207.21
$PbCl_2$: 2AgCl	207.22
Average	207.20

The close agreement of the chloride analyses with those of the bromide is very reassuring; for it is difficult to believe that any constant error, such as hydrolysis of the salts during fusion, could have affected both series of results to the same extent. Furthermore it is obvious that the different specimens of lead chloride are identical within the limit of experimental error, and that ordinary lead, if composed wholly or in part of isotopes, is astonishingly constant in composition.

The results of this investigation indicate (1) that lead bromide and chloride after fusion in atmospheres containing hydrobromic acid and hydrochloric acid respectively are neutral, (2) that no difference exists

between specimens of lead of different geographical origin, and (3) that the atomic weight of lead referred to silver as 107.880 is very nearly 207.20.

We are very greatly indebted to the Carnegie Institution of Washington for generous assistance in the pursuit of this investigation.[3]

[1] *Proc. Amer. Acad.*, 43, 365 (1907); *J. Amer. Chem. Soc.*, 30, 187; *Zeit. anorg. Chem.*, 57, 174.

[2] See, Richards and Lembert, *J. Amer. Chem. Soc.*, 36, 1329 (1914); M. Curie, *C. R. Paris Acad. Sci.*, 158, 1676 (1914); Hönigschmid and St. Horowitz, Ibid., 158, 1796 (1914); Soddy and Hyman, *J. Amer. Chem. Soc.*, 105, 1402 (1914).

[3] This work will be described in detail before long in *Proc. Amer. Acad. Arts Sci., Boston*, and in *J. Amer. Chem. Soc.*

A REVISION OF THE ATOMIC WEIGHT OF PRASEODYMIUM

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Presented to the Academy, December 16, 1914

Some years ago the atomic weight of neodymium was investigated in the Harvard Chemical Laboratory by the analysis of the chloride;[1] and the success met in obtaining pure material and in the method used, led us to investigate in a similar fashion the atomic weight of the twin element praseodymium. The International Committee upon Atomic Weights has chosen the value 140.6 for this constant, yet some of the recent investigations, notably those of Brauner, have yielded considerably higher results, in the neighborhood of 141.

Naturally, the first problem in the investigation was to prepare pure praseodymium salt. Through the great kindness of Dr. H. S. Miner of the Welsbach Light Company we were fortunate enough to secure as a starting point about 10 kg. of crude praseodymium ammonium nitrate, containing about 50% of the corresponding lanthanum and cerium salts as well as a small amount of neodymium. Since one of the most rapid and effective methods of freeing praseodymium salts from the closely related elements, lanthanum, cerium, neodymium, and samarium is the fractional crystallization of the double ammonium nitrate, we chose this method of purification. In this crystallization the bases separate in the order lanthanum, cerium, praseodymium, neodymium, samarium, and yttrium earths. The usual method of fractional crystallization was followed;[2] that is, the mother liquor of a less soluble fraction was used for dissolving the crystals of the nex more soluble fraction. The crystallization proceeded with the occasional rejection of fairly large fractions, chiefly lanthanum and cerium, at the less soluble end of each series, and the rejection of much smaller fractions, partly neodymium, at the more

soluble end, until 174 series of crystallizations had been made. The number of fractions in each series rapidly increased to a fairly constant number between 20 and 25. In series 174 an examination of the absorption spectra of the more soluble fractions showed that the extreme fraction contained a very small quantity of neodymium, not more than 0.05%, and that samarium was absent. At this point the seventh fraction from the more soluble end, 3474, was removed for analysis, since it seemed likely that this fraction was as pure as any in the series, and the more soluble fractions were rejected Fractional crystallization was then continued for 41 more series, without, however, rejecting any material at the more soluble end, although the least soluble fraction was occasionally discarded. The most soluble fraction of the 215th series, 4383, upon careful examination of the absorption spectrum, was still found to be free from neodymium, although it was found possible by the absorption spectrum to detect less than 0.05% of the latter element. This result indicates that the other fractions also were free from both neodymium and samarium. The least soluble fraction, 4368, was analyzed for cerium by precipitating the hydroxide and treatment with chlorine, and was found to contain 0.4%. The atomic weight of cerium is so nearly like that of praseodymium that even this proportion would lower the observed atomic weight of praseodymium less than 0.003 unit. Fraction 4371 was found to contain only 0.1% of cerium.

The quantitative examination of selected fractions of the last series was then undertaken. First, the double nitrate was converted into chloride. This was done by precipitating praseodymium oxalate, igniting the washed and dried product to oxide, dissolving the oxide in nitric acid, reprecipitating and igniting the oxalate, dissolving the oxide in hydrochloric acid in a quartz dish, and crystallizing the product from concentrated hydrochloric acid solution. The salt was then superficially dried in a vacuum desiccator

In order to dry the salt for weighing, it was carefully dehydrated by efflorescence at gradually increasing temperatures in a current of dry hydrochloric acid gas. The salt was contained in a weighed platinum boat, and this was placed in a quartz tube forming part of a Richards 'bottling apparatus.'[3] When essentially all the water had been expelled the temperature was raised rapidly until the salt was fused. After the salt had been allowed to cool the hydrochloric acid was displaced by dry nitrogen and this in turn by dry air. Then the boat was transferred to the weighing bottle without exposure to moisture, and it was weighed.

Upon treating salt thus prepared with pure water, sometimes a very small proportion of insoluble matter is found. The proportion of this material depends upon the care with which the salt is first dehydrated

previous to fusion. Careful analysis of this material showed it to be praseodymium oxychloride. By exercising every precaution in the drying of the salt and by fusing the salt as rapidly as possible, formation of the oxychloride can either be avoided completely or kept within a limit so small as to produce no important effect upon the final result. These precautions were taken in drying all the specimens of chloride.

The solution of the praseodymium chloride was precipitated in dilute solution with a solution of a weighed amount of the purest silver very nearly equivalent to the chloride. By examination of the mother-liquor with a nephelometer it was determined whether the amount of silver used was exactly equivalent to the chloride, and an initial deficiency or excess was made up with very dilute standard solutions. Finally the silver chloride was collected and weighed. Corrections were applied for moisture retained by the silver chloride and for the silver chloride dissolved in the washings.

The following table gives a comparison of the results obtained with the various fractions of the last series, as well as with the one selected from series 174, Fraction 3474. Of the others, Fraction 4383 is the mos 4368 the least soluble. As a rule 5 to 6 gram portions of the chloride were used. Vacuum corrections were applied to the apparent weights of praseodymium chloride, silver, and silver chloride.

It is apparent that the variations among the different fractions are chiefly fortuitous; for the extreme fractions examined show a difference which is hardly beyond the experimental error of analysis. The atomic weight of praseodymium seems to be very nearly 140.92, if silver is taken

THE ATOMIC WEIGHT OF PRASEODYMIUM

Ag = 107.880 Cl = 35.457

FRACTION	$PrCl_3$: 3Ag	$PrCl_3$: 3AgCl	AVERAGE VALUES
3474		140.913	
3474		140.939	
3474	140.943	140.923	
3474	140.924		
3474	140.914	140.941	
Average	140.927	140.929	140.928
4383	140.910	140.909	
4383	140.914	140.902	
Average	140.912	140.906	140.909
4381	140.908	140.937	
4381	140.920		
4381	140.915	140.925	
4381	140.921		

THE ATOMIC WEIGHT OF PRASEODYMIUM—Continued

FRACTION	$PrCl_3$: 3Ag	$PrCl_3$: 3AgCl	AVERAGE VALUES
Average.....	140.916	140.931	140.921
4379	140.910	140.913	
4379	140.915	140.928	
Average.....	140.913	140.921	140.917
4377	140.917		
4377	140.917	140.951	
Average.....	140.917		140.928
4374	140.916	140.965	
4374	140.916	140.943	
4374	140.959	140.952	
4374	140.926	140.911	
4374	140.911	140.916	
4374	140.911		
Average.....	140.923	140.937	140.930
4371	140.915	140.926	
4371	140.922	140.924	
Average.....	140.919	140.925	140.923*
4368	140.916	140.935	
4368	140.922	140.919	
Average.....	140.919	140.927	140.925†
Final Average.....			140.923
Average of all Individual Analyses.....			140.924

* Increased by +0.001 units to correct for cerium-content.
† Increased by +0.002 units to correct for cerium-content.

at 107.880. This result is not far from the value obtained by Brauner in both his researches, but is over three-tenths of a unit higher than the choice of the International Committee on Atomic Weights.

We are indebted particularly to the Carnegie Institution of Washington for very generous assistance in carrying out this investigation,[4] as well as to Dr. H. S. Miner of the Welsbach Light Company for the praseodymium material.

[1] Baxter and Chapin, *Proc. Amer. Acad.*, **46**, 213 (1911).

[2] The crystallization was begun by Mr. W. H. Whitcomb, continued by Messrs. B. W. Grimes and C. C. Wallace, and completed by O. J. Stewart.

[3] Richards, *Proc. Amer. Acad.*, **32**, 59 (1896).

[4] This work will be described in detail before long in *Proc. Amer. Acad. Arts Sci., Boston*, and in *J. Amer. Chem. Soc.*

ON THE EARTH CONSIDERED AS A HEAT ENGINE

By George F. Becker

UNITED STATES GEOLOGICAL SURVEY, WASHINGTON

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Imagine that the earth solidified in such a way that fluid equilibrium were perfectly preserved and that the exterior were perfectly smooth, presenting an ideal equipotential surface of uniform temperature. Suppose that the only differences between different portions of this surface were in the diffusivity (that there are large variations in the diffusivities of different rocks, the published determinations clearly show). For simplicity's sake, suppose that a certain square area had a uniform diffusivity smaller than that of the surrounding surface. It is evident that this square would cool more slowly than the adjacent portions, and by reason of the slower escape of heat it would develop a slight relative elevation—provided indeed that the material of the globe contracted in cooling as almost all substances actually do. Furthermore, the relative contraction of the surrounding mass would bring to bear a pressure on all four sides of the square, and this pressure would extend downward as far as the difference of temperature was sensible. Such a pressure might even suffice to rupture the rock within the square. Indeed the order of magnitude of the difference in temperature needful to induce rupture is easily estimated.

Let M be the Young's modulus of the rock composing the square, P the lateral pressure on any face and e the elongation. Then by definition $Me = P$ or $e = P/M$. The crushing load for the best granite is about 1000 kilograms per square centimeter, and the average Young's modulus found by H. Nagaoka[1] for 5 granites is 25×10^{10} dynes or 254800 kg. per square centimeter, only about half as great as that given by Everett for glass. This would indicate at the breaking point $e = 0.004$, very nearly. Now the linear expansion between 0°C. and 100°C. of mineral substances such as glass, quartz, topaz, etc., is not far from 0.08, or some 20 times the value for e just found. Hence a difference of temperature between the square and the surrounding area of the order of 5°C. would be enough to produce rupture in the square column imagined.

Thus a difference in diffusivity would bring about an intumescence on the surface of the globe followed by the formation of four systems of joints which in the simple case supposed would cross one another at right angles at the surface and dip at about 45° in four directions.[2] These joints are equivalent to voids, which as I have recently shown may in extreme cases approach 6.73 percent of the volume.[3] Thus a further

very considerable intumescence or uplift would result and an additional diminution in the diffusivity, because joints interfere with the conduction of heat. This diminution would further increase the depth to which sensible differences in temperature on the same horizontal plane would extend. Besides systematic rupture it is evident that the lateral pressures on the square column would or might bring about deformations and the crumpling of layers originally plane.

It would seem then that the initial difference in diffusivity need not be great eventually to ensure a considerable uplift within the hypothetical square since so soon as it sufficed to establish a temperature-difference of a few degrees the process of uplift would be increased by the effects of rupture.

Supposing no water to exist upon the earth, the square column under discussion might attain a notable elevation. The average land surface now stands about 4 km. or two and a half miles above the average sea bottom and if the material from which the salt of the ocean has been derived represents eroded continents of present area, the average land surface might have stood some 2.5 km. higher than it now does. Whether so lofty a mass could sustain its own weight need not be discussed here, the present object being merely to bring out a particular feature of the whole problem.

The mere configuration of the present continents standing at a mean elevation of nearly 13,000 feet above sea bottom manifestly represents a vast amount of energy of position, or potential energy, and to this must be added that of the total mass which has been eroded from the continents, something like half as much as now remains. (Thus, according to the computations made by F. W. Clarke in his *Data of Geochemistry*. the sodium of the ocean represents a spherical shell 2050 feet in thickness. If this is supposed piled onto continents of an area one-fourth that of the globe, its thickness would be 8200 feet, or somewhat more than one half of 13,000 feet.)

The internal temperature of the continental mass likewise calls for consideration. At the present day the mean depth of the ocean is about 3496 m., the means elevation of the land is about 440 m. and the mean thermometric gradient 1° in 38 m. or possibly as high as 1° in 32 m. The temperature at the bottom of the sea is not far from zero, while at the same level beneath the continents it is over 100°. Thus the mean temperature of the actual continents down to the level of sea bottom is more than 50°C. above the temperature of the sea-bottom itself, while for a long distance below this level the sub-continental masses must be hotter than the sub-oceanic layers. (Even

if no energy were dissipated within the continental plateaus, the rate at which heat would be conducted from the subterrestrial to the suboceanic regions would be exceedingly low.)

Some of this temperature-excess is probably a remnant of the original temperature of the globe equivalent to that which has failed to escape because of the low diffusivity of the continental rocks. Another portion, probably considerable, represents kinetic energy dissipated in the crushing and plication of the continental mass. Alas that we cannot evaluate the proportion!

On the hypothesis under discussion all of this temperature-excess and all of the energy of position represents heat which would have been radiated into space had all areas of the earth's surface been endowed with equal diffusivity.

The outer shell of the earth down to a depth of perhaps 70 or 80 miles at which the primeval temperature still prevails without sensible diminution may thus be regarded as an imperfect heat engine receiving heat energy at an absolute temperature approaching 2000° and emitting it at less than 300°. The difference is proportional to the energy which would be available were this engine perfect. Though far from perfect, it has sufficed, it seems to me, to supply what has been expended in maintaining in part the relatively high temperature of the sub-continental masses, and also in epeirogenic and orogenic upheavals, in the shattering and crumpling of rocks, and in earthquakes and volcanoes. (It is to be expected that the dissipation of energy would be peculiarly intense near the surface dividing the rising continental columns from the oceanic basins. It is in such positions that most of the volcanoes are found.)

So far the ocean has been practically ignored, but only a few years can have elapsed after the consistentior status before the sea came into being. Even a very small difference in diffusivity acting for a very short time would have served to outline depressions into which the incipient ocean would gather while, after a time at any rate, the presence of the ocean with its convective circulation would tend further to increase the difference in temperature between the areas of relatively great and relatively small diffusivity, which would then become oceanic basins and continental plateaus.

The moment an ocean formed, or rather a moment before it began to form, erosion commenced and introduced a new factor into the world system. Were the globe completely covered by the sea, evaporation and precipitation would furnish no energy of geological significance. The energy absorbed in evaporation would be liberated on precipitation and the molecules of water would return to their original level. But

water falling on a continent and carrying sediment to the sea depotentializes or sets free the energy of position acquired at the expense of the heat stored in the earth.

At great depths we know that rocks are deformed and undergo solid flow; and it is well known that under an appropriate system of stresses any solid must flow.[4] At the surface, so far as rocks are concerned, such a system of stresses does not exist, and the rocks do not flow. But erosive action lends them a mobility almost equivalent to fluidity so that the net result is in some respects analogous to that which would ensue if the solid surface of the globe were replaced by a mass of hyperviscous liquid some column or columns of which had a higher temperature than the surrounding matter. These columns would rise above the general surface because of the diminished density and the mounds thus formed would overflow or run down because they lack rigidity. The outflowing portions would cool, and sinking into general mass, would establish a convective circulation.

Not just in the same way, but similarly, erosion effects the flow of the continental surface matter to or beyond the edge of the continental plateaus overweighting the ocean floor and bringing about a corresponding subsidence.

In an asphalt lake like that of Trinidad, convection due to lack of temperature equilibrium would be attended by an undertow. Material rising from any particular depth would diminish the horizontal pressure which it had previously exerted on surrounding portions of the hyperviscous mass and these would press inwards to fill the partial void. In a solid earth there must be an analogous action, excepting that the partial pressure needful to produce lateral flow or undertow must exceed that which would strain the solid rock to its elastic limit.

The analogy of an asphalt lake must not be applied without caution. In such a lake it is easy to conceive of convective circulation indefinitely continued. Not so in the solid earth. If the whole rock mass from which the oceanic salt has been derived was really once piled on the continents and if the ocean is 100×10^6 years old then the total uplift of about $6\frac{1}{2}$ km. has only been effected at the rate of 1 mm. in 15 years or 1 inch in 380 years. Thus the process might be compared with *incipient* convection in an asphalt lake.

None the less, so far as it has gone, the undertow has tended to contract the area of incipient continents, to increase the crumpling and to exaggerate the elevation to which they would have attained had there been no convective tendency.

On the hypothesis that the origin of continents is due to the inferior diffusivity of certain areas of the earth's surface the condition of the ocean's bottom is very noteworthy. As is well known this floor is relatively featureless, consisting of vast plains, low ridges, and a few deeps, seven-eighths of its area lying at a depth of more than a kilometer below sea level. There are very few indications on the ocean floor of continental topography, and yet if a continent were to be submerged to a depth of a hundred fathoms or more, that is below the reach of wave action, it is difficult to see how any process of base-leveling could reduce its accentuation. Neither on the hypothesis under discussion is it easy to see how a continent could be submerged though it is barely possible that a thin layer of rock of small diffusivity might be removed by erosion leaving exposed masses of diffusivity so high as to undergo rapid contraction. Judging from the bathymetrical maps there are no important cases of this description, it would seem that, as the elder Dana so ably maintained, the oceanic areas have been persistent; and if so the subsidences which have occurred and recurred have been subordinate features of movements the net result of which in each case was uplift. This is in line with the results of Hayford, Helmert, and their associates. Since they have compelled us to concede that the earth is even now in a condition of approximate isostatic equilibrium, it seems impossible to believe that it has not been so in the past. Erosion has been in progress during every era from the Algonkian upward and there must have been a persistent and prevailing tendency to upheaval. Of a complete drowning of the continents, such as would occur during a prolonged era in which subsidence prevailed, there is now no trace.

Two reasons have been suggested above for the high level at which the continents stand relatively to the ocean floor, viz., superior temperature and the existence of voids. The difference in level is 3.936 km. or 0.032 of 122 km. If this difference were entirely due to excess of temperature, and if the linear expansion of average rock is 0.0008 per degree, the whole elevation of the continental columns would indicate a mean temperature difference of 40°C. If this elevation were due entirely to the existence of voids, these would amount to about half the maximum interstitial space found for me by Mr. Melcher in experiments on the crushing of sulphur in sealed brass tubes. It is evident that the two causes in combination might bring about elevations not only corresponding to the mean height of the continents but also to those of lofty mountain ranges.

Until erosion began the terrestrial mechanism must be regarded as a heat engine of the irreversible type. It could potentialize energy and do

mechanical work, but the cycle was incomplete. When erosion supervened and conferred upon the superficies of the continents a certain mobility and kinetic energy, the cycle was completed and the stage answering the adiabatic expansion was supplied.

It is true that the efficiency of this engine must be very small, but the store of energy upon which it draws—the available boiler capacity—is enormous. The mechanism thus appears competent to bring about all of the dynamical effects with which geology has to deal.

[1] H. Nagaoka, *Phil. Mag.*, **50**, 53 (1900).
[2] Simultaneous joints. *Proc. Wash. Acad.* **7**, 267 (1905).
[3] Becker, *J. Wash. Acad.*, **4**, 429 (1914).
[4] Ibbetson, *Math. Theory of perfectly elastic solids*, 1887, p. 174.

A PHYLOGENETIC STUDY OF CYCADS

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In any phylogenetic study it is desirable to compare related forms which are widely separated in time and space. In case of the cycads, such comparisons are becoming possible. The general plan of an investigation, begun more than ten years ago, is to study numerous forms in the field; study life histories in all parts of the family; and then, by comparative study, to gain some idea in regard to relationships and also in regard to the behavior of various structures during great periods of time.

A typical cycad, with its unbranched trunk and crown of pinnate leaves, looks like a tree fern, an aspect which dates back as far as anything is known about the phylum.

In the Carboniferous, two great lines of seed plants were abundant and widely distributed. One of them is represented today by such familiar forms as the pines, junipers, araucarias and taxads. They are mostly evergreen, are widely distributed, and form immense forests. Being easily available, they have been thoroughly studied, especially the north-hemisphere genera. The other line is represented by a single living family, the Cycadaceae, or cycads, as they are commonly called. The Paleozoic ancestors of the cycads had a world-wide distribution and made such a display that botanists have called the Paleozoic the Age of Ferns, since the leaves were very naturally mistaken for fern leaves. These fern-like seed plants undoubtedly came from an even earlier fern ancestry and, in the Carboniferous, existed side by side with

true ferns; but, whether a given specimen should be assigned to the ferns or to these primitive seed plants, cannot be determined by the leaves alone. The only decisive character is the seed.

From splendidly preserved material, English botanists have been making a vigorous study of this group, so that their results are available for comparison. Their researches have covered the external form and also the internal structure of the stem, leaf, root, microsporangium and megasporangium; but, as yet, almost nothing is known about the gametophytes and embryo.

A Mesozoic order—known as the Bennettitales—which may have been the predecessor of the cycads, was also fern-like in appearance. These forms still retained the unbranched trunk and crown of pinnate leaves, so characteristic of ferns and Paleozoic seed plants; but only pinnae of the simplest type persisted and the trunk was short and stocky, so that in general appearance they bore more resemblance to the modern cycads than to their Paleozoic ancestors. The microsporangia were borne on a whorl of leaves, reduced considerably, but still retaining the pinnate character of the foliage leaves. The seed bearing structures, however, were collected into a compact cone, in which no one but a morphologist would recognize the homologues of leaves. Both the pollen producing and the ovule bearing leaves were in the same strobilus, so that the bisporangiate strobilus is a striking feature of the group, or at least, of its upper Mesozoic members. These strobili were small and numerous and were borne in the axils of foliage leaves. The material which has been under investigation has come from Idaho, Dakota, Maryland, Mexico, Europe and India; but this does not mean that the group was confined to the northern hemisphere, for these places are all in the university belt or are easily reached from universities. Forms which may belong here, or may belong to the true cycads, occur in Cretaceous deposits in South Africa, and vast regions remain to be explored.

G. R. Wieland has made the most extensive contributions in this field, but, as in case of the Paleozoic members of the phylum, scarcely anything is known of the gametophytes and not much is known about even the sporophyte structures of Triassic forms.

While I have studied, with great interest, the best material of the Paleozoic and Mesozoic forms, my own research has been confined to the only living family of the phylum, the Cycadaceae, or cycads.

The geological history of the cycads is still very indefinite, largely because stem and leaf characters are hardly sufficient to distinguish true cycads from the Mesozoic Bennettitales. The confident determina-

tions which one sees in museums bring to mind the so-called ferns of the Paleozoic. However, some of the living genera are distinguishable in the Tertiary and it is possible that the family goes back even to the Triassic. The resemblance of the stems and leaves of the living cycads to those of the Bennettitales is too striking to be accidental; on the other hand, there is nothing in the seed-bearing cones of any of the Bennettitales yet described which could have given rise to the seed-bearing cones of the living cycads. Further investigation, especially of Triassic forms, must be made before one can say with any confidence whether the modern cycads have come from the Bennettitales, or both have come from the Paleozoic Cycadofilicales.

The geographic distribution of the living cycads is very restricted. All are tropical or subtropical. Four of the nine genera belong to the western hemisphere and five to the eastern, there being no genus common to both hemispheres. With two exceptions, the genera are very local in their distribution. In the western hemisphere, the thirty-odd species of *Zamia* range from Florida to Chili; while, in the eastern hemisphere, the dozen species of *Cycas* range from Japan to Australia. Two genera are found only in Mexico; one only in Cuba; two only in Australia, one of these being confined to Queensland; and the other two are found only in South Africa. So, with the exception of the two genera which cross the equator, the western genera are in the northern hemisphere and the eastern genera are in the southern. Even in the places mentioned, the plants occur singly or in scattered groups. There is no such things as a cycad forest.

Perhaps, later, it may be possible to give some reason for this peculiar distribution and for the scarcity of individuals.

The nine genera of living cycads are so sharply defined that there is no difficulty in recognizing them. In herbarium specimens, this rigidity extends to species, and, before I began to study cycads in the field, I supposed that the species were rather sharply marked. By the time I had made four excursions into the Mexican tropics, it was evident that species could vary, even in *Dioon* and *Ceratozamia*. However, I was not prepared for the bewildering maze of forms which I found as I tramped the *Macrozamia* regions of Australia. Three species, with erect cylindrical trunks, are easily distinguishable, but I was almost tempted to say, "Call the rest *Macrozamia spiralis*, and let them go." Taxonomists describe several species of the South African genus, *Stangeria*, but, after wandering over nearly the entire range of the genus, from Zululand to Port Elizabeth, I should not be surprised if all of them should be raised from the seeds of a single cone. Some species of the African

genus, *Encephalartos*, seem comfortably distinct, while others are so variable that one could identify herbarium specimens with more assurance than he could plants in the field.

An experimental study of variation in cycads is not attractive. *Dioon edule* probably reaches an age of fifty years before it produces cones; *Macrozamia Moorei* has been known to cone at twenty years; some species of *Zamia* may cone at ten years, or even less; but the succession of generations is too slow for any experimental work involving the whole life history. Seedlings of all the genera and many of the species are growing at the University of Chicago and it is evident that there is considerable variation in the seedlings from a single cone. Some work in hybridization is being attempted and one might reasonably hope to see results if any should appear in the first generation.

In comparing Paleozoic, Mesozoic and living members of the phylum, it is evident that the Mesozoic forms are larger than the Paleozoic, and that the living forms are still larger than the Mesozoic. Some of the seeds of the Paleozoic forms were very small, but some were larger than any yet known in the Mesozoic, yet none even approached the large seeds of some of the living cycads. Cones of the Bennettitales can be carried in the pocket, but some of the cones of living cycads reach a weight of ninety pounds. While the sperms of the fossil forms have never been identified with certainty, the structure of the ovules makes it certain that they could not have been nearly so large as those of the living cycads. On the whole, there has been an increase in size as we pass from the Paleozoic to the living forms.

In all the living Gymnosperms, the development of the female gametophyte begins with a series of free nuclear divisions which is followed by cell formation. Undoubtedly, the lowest seed plants came from heterosporous Pteridophytes, and it is more than probable that these heterosporous Pteridophytes of the Paleozoic had female gemetophytes beginning with free nuclear division, although the earliest heterosporous forms, just emerging from the homosporous condition, probably had female gametophytes in which all nuclear division was accompanied by the formation of walls. No homosporous form, either living or fossil, shows free nuclear division at this stage in the life history. Free nuclear division came with the increase in the size of the spore, and the extent of the free nuclear period is more or less correllated with the size of the spore. In the large megaspores of *Dioon* there may be thousands of free nuclei before wall formation begins; in the smaller megaspores of *Zamia*, only hundreds.

That the theory, just expressed, is correct, is indicated by the early development of the sporophyte in Gymnosperms. The eggs are large, and—with two or three exceptions—fertilization is followed by a period of free nuclear division before walls begin to appear. In the two or three exceptions, the egg is small, and the first division of its nucleus is followed by the formation of a wall. In all Angiosperms, the egg is small and a wall follows the first division of its nucleus.

Thus we have a series beginning with the homosporous Pteridophytes in which a wall always followed the first division of the spore, free nuclear division appearing somewhere, as heterospory was attained and the megaspore increased in size; the free nuclear period becoming more prolonged in the larger megaspores of the Gymnosperms, then culminating and beginning to decline in the cycads. The series is even more striking in case of the embryo, since there is the same increase, culmination and decline of the free nuclear period and a few Gymnosperms, together with all Angiosperms have come back to the original condition in which a wall follows the first nuclear division of the egg.

Some of the microscopic details are remarkably constant, e.g., in the pollen tube of all the genera the prothallial cell presses deeply into the body cell. The cycads can be distinguished from all other living seed plants by this cytological character. In all the genera, two blepharoplasts appear in the body cell and, in the two sperms derived from it, develop into coiled bands bearing thousands of cilia by means of which the sperm becomes a vigorous swimmer. Since these details are so uniform, they must antedate the differentiation into the modern genera. On the other hand, there are characteristic differences in the pollen tubes and their contents, so that the pollen tube structures will not only distinguish the family from other seed plants, but will distinguish the genera from each other.

The foregoing paragraphs are intended to indicate the drift of a few phases of an investigation which has been in progress for more than ten years. So far, only scattered descriptive sketches have been published, but all the genera and many of the species have been studied in the field and material has been collected for detailed studies of practically all phases of the life histories. Besides, obliging correspondents in Cuba, Mexico, Australia and Africa are constantly sending field notes, photographs and material, so that, in time, the interrelationships of the genera and the origin of the family may be cleared up and some opinion may be ventured in regard to phylogenetic characters and their gradual modification.

DIMORPHISM IN SIZE OF SPERMATOZOA AND ITS RELATION TO THE CHROMOSOMES

By Charles Zeleny and E. C. Faust

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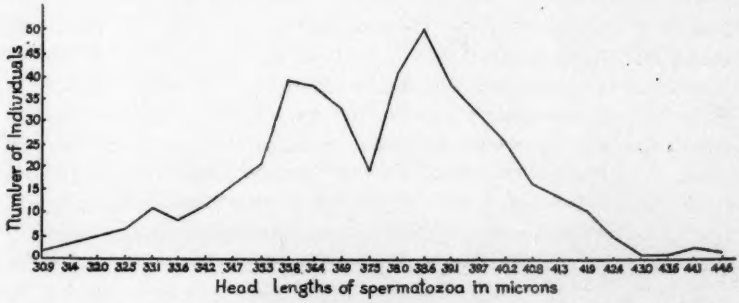
Cytological studies have shown that in the maturation divisions of a large number of species of animals the cells which are to give rise to the spermatozoa are of two kinds, differing from each other in the nature of their chromatin content. The resulting spermatozoa therefore are assumed to be likewise of two kinds. A large mass of indirect evidence makes it highly probable that eggs fertilized by one of these produce only males and those fertilized by the other only females.



EXPLANATION OF FIGURE

Frequency distribution of head-lengths of 444 spermatozoa from right testis of Musca domestica.

Value in microns	30.9	31.4	32.0	32.5	33.1	33.6	34.2	34.7	35.3	35.8	36.4	36.9	37.5
Frequency	1	3	5	6	11	9	12	16	20	39	38	33	20
	38.0	38.6	19.1	39.7	40.2	40.8	41.3	41.9	42.4	43.0	43.5	44.1	44.6
	40	50	39	33	27	17	14	11	5	1	1	2	1

Direct experimental test of this hypothesis has not been made because recognition of the difference has been confined to the immature stages. It is therefore of importance to see if the two kinds can be made out among the completed functional spermatozoa. The present studies were planned with a view to such a determination. If there is a difference in the amount of chromatin received by the two kinds of spermatids it is natural to expect a corresponding difference of size in the heads of the completed spermatozoa since these are made up almost wholly of chromatin material.

The head-lengths of spermatozoa from fifteen species of animals were measured. The number of measurements for a single determination ranged from 322 to 1008 with an average of 523, including in each case only the spermatozoa from a single testis. A considerable variation in length was present and when the frequency distributions were plotted the resultant curves were found to be distinctly bimodal in fourteen of the fifteen species. One of these curves is shown in the accompanying figure. The complete data are summarized in a table. The inference is drawn that there are two groups as regards size among the functional spermatozoa of these species, the group with the larger individuals in each case consisting of those which the studies in spermatogenesis have shown to have the larger chromosomal volumes.

The species for which these determinations have been made belong to several animal groups:

Diptera:	Musca domestica, three determinations.
Heteroptera:	Lygaeus palmii, two determinations.
	Alydus pilosulus, two determinations.
	Anasa tristis, ten determinations.
Coleoptera:	Trirhabda tomentosa, one determination.
	Phytonomus punctatus, two determinations.
Orthoptera:	Melanoplus femur-rubrum, one determination.
	Melanoplus differentialis, three determinations.
	Gryllus abbreviatus, one determination.
Odonata:	Aeshna canadensis, one determination.
Amphibia:	Rana pipiens, one determination.
Reptilia:	Pseudemys troosti, two determinations.
Mammalia:	Ovis aries, one determination.
	Bos taurus, one determination.
	Canis familiaris, one determination.

In all but one of these species two size-groups were recognized. The exception is the one determination of Gryllus abbreviatus. Chromosomal dimorphism has been described for all of the groups except the Amphibia, in most of the cases for the particular species here studied.

A further proof of the close relation between the chromosomal dimorphism of the spermatids and the dimorphism in size of the adult spermatozoa was sought in a determination of the volume of chromosomal material in each of the two kinds of spermatids. From the volumes obtained the expected ratio between the head-lengths of the resultant spermatozoa was calculated under the assumption that the volume of the heads is directly proportional to the amount of the chromatin received and that the shape is the same in the different sizes.

For three of the species studied, Musca domestica, Alydus pilosulus and Anasa tristis, there are good published figures of the chromosomes in the spermatocyte divisions. From these figures it is possible to make a rough estimate of the total chromosomal volume in each of the two kinds of cells. The expected ratio between the head-lengths of the two kinds of resulting spermatozoa was calculated from these data. A close agreement was discovered between the calculated ratios and the observed ratios. For Musca domestica the calculated ratio is 1.00 : 1.08 and the average of the observed ratios 1.00 : 1.07. For Alydus pilosulus these ratios are 1.00 : 1.06 and 1.00 : 1.055. For Anasa tristis the calculated ratio is 1.00 : 1.11 and the average of the observed ratios 1.00 : 1.09 with six of the nine determinations located between 1.00 : 1.10 and 1.00 : 1.12

While there undoubtedly is a considerable error in making minute measurements like those here described and while there may be doubt as to the significance of some of the individual determinations yet the data when taken as a whole must be interpreted as showing the very general existence of dimorphism in size among the completed spermatozoa. The evidence further makes it very probable that this dimorphism is the result of the chromosomal dimorphism present in the spermatids. If this conclusion is a correct one the hypothesis that the chromosomal dimorphism is related to sex determination may be subjected to direct test as soon as a method can be devised for separating the different sizes of living spermatozoa.

TABULATION OF DATA ON HEAD-LENGTHS OF SPERMATOZOA

The observed ratio is the ratio between the two modal values. The expected ratio is calculated from the chromosomal difference between the two kinds of spermatids.

(over)

NAME	NUMBER OF SPERMATOZOA	HEAD-LENGTH LOWER MODE IN MICRONS	HEAD-LENGTH UPPER MODE IN MICRONS	OBSERVED RATIO	EXPECTED RATIO
1. Musca domestica	444	35.8	38.6	1.00 : 1.08	1.00 : 1.08
2. Musca domestica	465	35.8	38.6	1.00 : 1.08	1.00 : 1.08
3. Musca domestica	769	36.4	38.0	1.00 : 1.04	1.00 : 1.08
4. Lygaeus turcicus					1.00 : 1.02
5. Lygaeus palmii	493	36.8	38.3	1.00 : 1.04	
6. Lygaeus palmii	501	36.8	37.8	1.00 : 1.03	
7. Alydus pilosulus	429	31.9	33.9	1.00 : 1.06	1.00 : 1.06
8. Alydus pilosulus	469	32.25	33.75	1.00 : 1.05	1.00 : 1.06
9. Anasa tristis	653	28.35	30.2	1.00 : 1.07	1.00 : 1.11
10. Anasa tristis	391	17.5	30.2	1.00 : 1.10	1.00 : 1.11
11. Anasa tristis	370	28.1	30.8	1.00 : 1.10	1.00 : 1.11
12. Anasa tristis	443	17.5	30.8	1.00 : 1.12	1.00 : 1.11
13. Anasa tristis	404	28.1	30.8	1.00 : 1.10	1.00 : 1.11
14. Anasa tristis	384	28.6	30.8	1.00 : 1.08	1.00 : 1.11
15. Anasa tristis	394	28.1	30.8	1.00 : 1.10	1.00 : 1.11
16. Anasa tristis	322	30.8	31.8	1.00 : 1.03	1.00 : 1.11
17. Anasa tristis	993		31.5		1.00 : 1.11
18. Anasa tristis	444	28.4	31.5	1.00 : 0.11	1.00 : 1.11
19. Trirhabda virgata					1.00 : 1.06
20. Trirhabda tomentosa	481	17.02	17.78	1.00 : 1.045	
21. Phytonomus punctatus	506	33.0	34.0	1.00 : 1.03	
22. Phytonomus punctatus	507	33.3	35.3	1.00 : 1.06	
23. Melanoplus femur-rubrum	491	80.5	83.0	1.00 : 1.03	
24. Melanoplus differentialis	481	88.0	90.5	1.00 : 1.03	
25. Melanoplus differentialis	1008	88.8	90.5	1.00 : 1.02	
26. Melanoplus differentialis	734	88.5	90.9	1.00 : 1.03	
27. Gryllus abbreviatus	552				
28. Anax junius					1.00 : 1.07
29. Aeshna canadensis	496	50.2	50.6	1.00 : 1.03	
30. Rana pipiens	494	9.74	10.6	1.00 : 1.09	
31. Pseudemys troosti	501	10.43	10.91	1.00 : 1.05	
32. Pseudemys troosti	487	10.50	11.36	1.00 : 1.08	
33. Ovis aries	498	5.94	6.37	1.00 : 1.07	
34. Bos taurus	606	8.05	8.33	1.00 : 1.035	
35. Canis familiaris	540	5.17	5.55	1.00 : 1.07	

AN INTERPRETATION OF SELF-STERILITY

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In certain hermaphroditic animals and plants, self-fertilization is often impossible. This gametic incompatibility has been called self-sterility. In the vegetable kingdom it is known to be comparatively widespread; in the animal kingdom, though it may be found later to be characteristic of many species, as yet only the Ascidian *Ciona intestinalis* has furnished material for study of the problem. (See Morgan,[1] Adkins, in Morgan,[2] and Fuchs.[3])

Ciona is not perfectly self-sterile. Individuals appear to vary in degree of self-sterility, though no case has yet been found where self-fertility is equal to cross-fertility. Morgan believes that there is a great difference in the compatibility of ova to sperm from other individuals, though Fuchs maintains that 100% of segmenting eggs can be obtained in every cross with normal ova if a sufficiently concentrated sperm suspension is used.

Fuchs has shown a chemical basis for the phenomenon by the difference in ease of cross-fertilization after contact of ova with sperm from the same animal and by the variation in ease of self-fertilization after certain artificial changes in the chemical equilibrium of the medium surrounding the ova, and by this work has brought the matter of self-sterility in Ciona in line with that in Angiosperms as worked out by Jost.[4]

Jost has shown that in the plants with which he worked only short tubes were formed after pollination with pollen from the same plant, though the necessary length of pollen-tube was easily developed after cross-fertilization. He saw as cause of these phenomena a chemotropism due to the presence of 'individueller Stoffe.' Pollen was indifferent to 'Individualstoff' from the same plant, but was stimulated by that from other plants.

To Correns[5] such an explanation of self-sterility seemed too general. He believed that a simple interpretation would account for the results he had obtained from *Cardamine pratensis*. Two plants B and G were crossed reciprocally and sixty of the offspring tested by pollinating from the parents, on the parents and from sisters. The back crosses of (B × G) or (G × B) with B and with G seemed to him to indicate four equal-sized classes with reference to gametic compatibility: (1) plants fertile

with both B and G; (2) plants fertile with B but not with G; (3) plants fertile with G but not with B; (4) plants fertile with neither B nor G.

These facts were interpreted by assuming the existence of two independently inherited factors that inhibit the growth of pollen-tubes. Representing these factors by the letters B and G, the original plants must be supposed to have had the formulae Bb and Gg respectively, since it is clear that type BB and GG could never be formed. When Bb is crossed with Gg the four types BG, Bg, bG and bg should result, of which the first three should be self-sterile. Plants BG should be fertile with plants bg, plants Bg should be fertile with bG and bg, plants bG should be fertile with Bg and bg, while plants bg should be self-fertile as well as cross-fertile with the other three classes. Attractive as this theory is, it is not clearly in accord with the facts. Plants of the type bg—inherently self-fertile—were not found, and the other classes showed many discrepancies.

Morgan[2] has offered another hypothesis that fits the data from both plants and animals. If I have not misunderstood the meaning of his rather general statement of the proposition, my own theory is only an extension of it, laid down perhaps a little more specifically. He says:

> The failure to self-fertilize, which is the main problem, would seem to be due to the similarity in the hereditary factors carried by the eggs and sperm; but in the sperm, at least, reduction division has taken place prior to fertilization, and therefore unless each animal was homozygous (which from the nature of the case cannot be assumed possible) the failure to fertilize cannot be due to homozygosity. But both sperm and eggs have developed under the influence of the total or duplex number of hereditary factors: hence they are alike; i.e., their protoplasmic substance has been under the same influence. In this sense, the case is like that of stock that has long been inbred, and has come to have nearly the same hereditary complex. If this similarity decreases the chances of combination between sperm and eggs we can interpret the results.

My own work has been done with the descendants of a cross between *Nicotiana forgetiana* (Hort) Sand., a small red-flowered species, and *Nicotiana alata* Lk. and Otto. var. *grandiflora* Comes, the large white-flowered sort commonly known as *Nicotiana affinis*. Both parents were undoubtedly self-sterile as over 500 plants of the F_1, F_2, F_3, and F_4 generations have been found to be self-sterile by careful tests.

Several experiments were made in which selfing, crossing *inter se*, and back crossing were done on a large scale, using plants of the F_2,F_3 and F_4 generations which had segregated markedly in size and were of

at least eight different shades of color. In the F_2 generation, twenty plants coming from two crosses between F_1 plants were selected for experiment. Each was selfed many times and in addition 131 inter-crosses were attempted, from four to twelve flowers being used in each trial. All attempts at selfing failed, while only two attempts at crossing were unsuccessful. Of the 129 successful inter-crosses, all but 4 produced full capsules, and it is probable that even this variability in ease of cross-fertilization was caused by attending conditions. One hundred and twenty other inter-crosses were made in the F_2 generation, with three failures.

In the F_3 generation, about one hundred inter-crosses were made between twelve plants which were the progeny of two sister F_2 plants. Six of these attempts failed.

In the F_4 generation, fifty-eight inter-crosses were made between ten plants that were the daughters of two F_3 plants. Fifty-three of these cross-fertilizations were successful.

Back crosses also were made in considerable numbers, though not to the extent one might desire. Plants A, B, C and D were combined in four different ways and among the plants resulting from these combinations eighty-five back crosses were attempted, of which five failed.

These facts will not fit any simple Mendelian formula similar to that proposed by Correns; furthermore, data from an experiment of a different kind appear to support Jost's idea of 'Individualstoffe' rather than Correns' idea of inhibitors. Pairs of plants were provided to furnish series of selfed and crossed flowers. The pistils of these flowers were fixed at regular periods after pollination, stained, sectioned and the pollen-tubes examined. Since the flowers on each plant had about the same length of pistils, curves of pollen-tube development for both crossing and selfing could be constructed. The pollen grains germinated perfectly on stigmas from the same plant, from 1200 to 2000 tubes having been counted in sections of single pistils. The difference between the development of the tubes in the selfed and the crossed styles was wholly one of rate of growth. The tubes in the selfed pistils developed steadily at a rate of about 3 mm. per twenty-four hours, with even a slight acceleration of this rate as the tubes progressed. If the flowers were sufficiently long-lived, one could hardly doubt but that the tubes would ultimately reach the ovules, though this would not necessarily mean that fertilization must occur. Since the maximum life of the flower is about 11 days, however, the tubes never traverse over one-half of the distance to the ovary. On the other hand, the tubes in the crossed pistils, though

starting to grow at the same rate as the others, pass down the style faster and faster, until they reach the ovary in four days or less.

From these facts it seems reasonable to conclude that the secretions in the style stimulate the pollen-tubes from other plants instead of inhibiting the tubes from the same plant.

The whole question, therefore, becomes a mathematical one, that of satisfying conditions whereby no stimulus is offered to pollen-tubes from the same plant, but a positive stimulus is offered to tubes from *nearly* every other plant.

The nearly constant rate of growth of pollen-tubes in the pistils of selfed flowers, compared with the regular acceleration of growth of the tubes from the pollen of other plants, undoubtedly shows the presence of stimulants of great specificity akin to the 'Individualstoffe' of Jost, though I believe their action to be indirect. Experiments by several botanists, which I have been able partially to corroborate, point to a single sugar, probably of the hexose group, as the direct stimulant. The specific 'Individualstoffe' I believe to reside in the pollen grains and to be in the nature of enzymes of slightly different character, all of which except the one produced by the plant itself for the use of its own pollen, or by another individual of the same genotype, can call forth secretion of the sugar that gives the direct stimulus. At least this idea links together logically the fact of the single direct stimulus and the need of 'Individualstoffe' to account for the results of the crossing and selfing experiments But whether or not this be the correct physiological inference, the crossing and selfing experiments call for an hypothesis that will account for no stimulation being offered the tubes from the plant's own pollen while at the same time great stimulation will be given the tubes from the pollen of *nearly* every other plant. This is a straight mathematical problem, and it is hardly necessary to say that it is insoluble by a strict Mendelian notation such as Correns sought to give. This is obvious to anyone familiar with the basic mathematics of Mendelism. On the other hand a near Mendelian interpretation satisfies every fact.

Let us assume that different hereditary complexes stimulate pollen-tube growth and in all likelihood promote fertilization, and that like hereditary complexes are without such effect. One may then imagine any degree of heterozygosis in a mother plant and therefore any degree of dissimilarity between the gametes it produces, without there being the possibility of a single gamete having anything in its constitution not possessed by the somatic tissues of the mother plant. From the chromo-

some standpoint of heredity the cells of the mother plant are duplex in their organization: they contain N pairs. The cells of the gametes contain N chromosomes, one coming from each pair of the mother cell; but they are all parts of the mother cell and contain nothing that that cell did not contain. These gametic cells cannot reach the ovaries of flowers on the same plant because they cannot provoke the secretion of the direct stimulant from the somatic cells of that plant.

All gametes having in their hereditary constitution something different from that of the cells of a mother plant, however, can provoke the proper secretion to stimulate pollen-tube growth, reach the ovary before the flower wilts, and produce seeds. Such differences would be very numerous in a self-sterile species where cross-fertilization must take place; nevertheless like hereditary complexes in different plants should be found, and this should account for the small percentage of cross-sterility actually obtained. It must be granted that this hypothesis satisfies the facts, but that is not all. It is admittedly a perfectly formal interpretation, but from a mathematical standpoint—granting the generality of Mendelian inheritance—it is the only hypothesis possible that can satisfy the facts.

In conclusion it should be mentioned that the cross-pollinated pistils show a considerable variation in the rate of growth of individual pollen-tubes, though our curves of growth have been made by taking the average rate of elongation. Is this variation a result of chance altogether or must one assume a differential rate of growth increasing directly with the constitutional differences existing between the somatic cells and the various gametes? If it is assumed that any constitutional difference between the two calls forth the secretion of the direct stimulus to growth, chance fertilization by gametes of every type different from that of the mother plant will ensue; if there is a differential rate, selective fertilization will occur. This question cannot be decided definitely at present, but two different lines of evidence point toward chance fertilization:

1. Flowers from a single plant pollinated by different males show no decided difference in rate of fertilization.

2. Color differences are transmitted to expected ratios.

Further, it will be recalled that beginning with the F_2 generation sister plants crossed together have given us our F_3 and F_4 populations, and that these F_3 and F_4 populations apparently have given a constantly increasing percentage of cross-sterility. This is what should be expected under the theory that a small difference in germ plasm constitution is as active as a great difference in causing the active stimulation to pollen tube

growth. Breeding sister plants together in succeeding generations causes an automatic increase of homozygosity as is well known. This being a fact, cross-sterility should increase. Such an increase in cross-sterility has been observed in the F_3 and the F_4 generations, but it would not be wise to maintain dogmatically that it is significant.

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[2] Morgan, T. H., *Heredity and Sex*. New York. Columbia University Press, ix + 1–282 (1913). (page cited 217).

[3] Fuchs, H. M., On the conditions of self-fertilization in Ciona. *Archiv. f. Entwickl. d. Org.*, 40, 157–204 (1914); The action of egg-secretions on the fertilizing power of sperm. *Archiv. f. Entwickl. d. Org.*, 40, 205–252 (1914).

[4] Jost, L., Zur Physiologie des Pollens. *Ber. d. deut. Bot. Ztg., Heft V and VI* (1907).

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THE BASAL CALORIC OUTPUT OF VEGETARIANS AS COMPARED WITH THAT OF NON-VEGETARIANS OF LIKE WEIGHT AND HEIGHT

By Francis G. Benedict and Paul Roth

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Presented to the Academy, January 13, 1915

Among the numerous, more or less scientifically attested, facts concerning the influence of a vegetarian diet are statements relative to an observed increase in endurance and the belief that vegetarians live upon a somewhat lower metabolic plane than do flesh eaters, who, it is asserted, are unduly stimulated by the protein in their food. The measure of the basal gaseous metabolism, which may be considered as the carbon-dioxide production and oxygen consumption during complete muscular repose and in the post absorptive condition, that is, at least 12 hours after the last meal, gives an admirable index of the metabolic activity.

We have been able to make observations on the basal gaseous metabolism of 11 men and 11 women, who had subsisted upon a vegetarian diet for a considerable period of time, extending, on the average, over several years. With the subjects in the post absorptive condition and lying quietly upon a comfortable couch, the total carbon-dioxide production and oxygen consumption of each of these individuals were measured on several days by means of the universal respiration apparatus. As a rule the values obtained agree well with each other and the averages

may be looked upon as reasonably accurate indices of the actual basal metabolism from which the energy output may be computed.

In order to study particularly the influence of a vegetarian diet, it was necessary to compare these values for vegetarians with those obtained on non-vegetarians of the same height and weight. A sufficiently large number of observations on non-vegetarians, including both men and women, was available for a satisfactory comparison of this kind. The body weights of the men ranged from 75 kg. to 49.3 kg. and of the women from 93.6 kg. to 40.0 kg. A comparison of the heat production per 24 hours as computed from the gaseous exchange showed that the men vegetarians produced 25.5 calories per kilogram and the non-vegetarian men of like height and weight 26.4 calories. On the commonly used, yet questionable, basis of the heat per square meter of body surface per 24 hours the vegetarians showed 798 calories as compared with 828 calories for the non-vegetarians. With the women the corresponding values per kilogram of body weight were 24.6 calories for the vegetarians and 25.0 calories for the non-vegetarians; and per square meter of body surface 753 calories for the vegetarians compared with 766 calories for the non-vegetarians.

Believing that the relatively large proportion of carbohydrate supposedly eaten in the vegetarian diets might tend to a larger storage of body glycogen, thus giving available carbohydrate material to be drawn upon in the endurance and similar tests of muscular efficiency, a computation was made in all cases of the respiratory quotient, i.e., the relationship between the carbon-dioxide production and oxygen consumption. When the katabolism is exclusively from carbohydrate this quotient is 1.0; with pure fat it is 0.70. For the 22 vegetarians (11 men and 11 women) the average quotient was found to be 0.83 while the average for the 132 non-vegetarians (77 men and 55 women) it was 0.81. The mathematical difference between these average respiratory quotients is too slight to be taken as evidence of a larger glycogen storage.

The results show, therefore, that the gaseous metabolic processes of the vegetarians are qualitatively and quantitatively essentially those of non-vegetarians of similar height and weight with whom they are compared.

The detailed report of this study has been transmitted to the *Journal of Biological Chemistry*.

THE INFLUENCE OF ATHLETIC TRAINING UPON BASAL METABOLISM

By Francis G. Benedict and H. Monmouth Smith

NUTRITION LABORATORY, CARNEGIE INSTITUTION OF WASHINGTON AND CHEMICAL LABORATORY, SYRACUSE UNIVERSITY

Presented to the Academy, January 13, 1915

Two fundamental conceptions exist with regard to the heat production of the body. One is that the heat production is proportional to the active mass of protoplasmic tissue; the other, and more generally accepted view, is that the heat production is proportional to the surface area of the body. To throw light upon this question we have made an investigation upon the metabolism of a number of trained college athletes in whom the proportion of active protoplasmic tissue would undoubtedly be greater than that in the body of a normal, untrained individual. In considering the results of this study, the differences in surface area have been eliminated by making the comparisons only between groups of individuals with like height and weight.

Fifteen athletes, varying in weight from 88.5 kg. to 56.3 kg., were studied by means of the universal respiration apparatus, the subjects being in a condition of complete muscular repose and in the post absorptive state, i.e., at least 12 hours after the last meal. Only the average values of several closely agreeing tests with each subject are used in the comparisons. From an extended series of metabolism experiments on normal individuals a sufficient number of suitable non-athletes of similar height and weight are selected for a series of comparisons which are made in ten groups. Of these ten groups the first four are given in the table herewith, thus serving to indicate the method of comparison as well as the actual values found in the four groups. In the complete comparison we found that in the ten groups the heat production per kilogram of body weight was greatest with the athletes in all but two groups, and in these two there was no difference between the athletes and non-athletes. The heat production per kilogram per 24 hours averaged for the athletes 26.0 calories and for the comparable non-athletes, 24.4 calories.

Per square meter of body surface the athletes again exceeded the metabolism of the non-athletes in all groups with but slight increases in 3 groups. The average heat production per square meter per 24 hours was 863 calories with the athletes and 807 calories with the non-athletes.

In general there was a distinct tendency for the athletes to have a measurably larger basal resting metabolism (about 7%) than the non-

COMPARISON OF THE HEAT PRODUCTION OF ATHLETES AND NORMAL NON-ATHLETIC MEN.

Group and subject	Nude weight	Height	HEAT PRODUCTION PER 24 HOURS (computed)		
			Total	Per kilogram	Per square meter
	kilos	*cms.*	*cals.*	*cals.*	*cals.*
Group I					
Athlete					
W. S.	88.5	165	2017	22.8	823
Non-athlete					
O. F. M.	85.8	171	1827	21.3	761
Group II					
Athletes					
J. H. R.	82.2	187	1978	24.1	849
D. H. W.	82.1	186	2034	24.8	873
M. H. K.	79.0	188	1944	24.6	856
E. G.	78.9	184	2126	27.0	940
Non-athletes					
F. G. B.	83.1	183	1802	21.7	770
W. A. M.	78.0	183	1816	23.3	807
Group III					
Athlete					
F. G. R.	74.0	179	1914	25.9	882
Non-athletes					
W. J. T.	74.2	183	1770	23.9	816
C. B. S.	71.1	179	1700	23.9	806
Group IV					
Athletes					
C. D. R.	74.0	173	1908	25.8	879
H. R. W.	73.9	175	1842	24.9	848
Non-athletes					
Dr. M.	75.9	175	1877	24.7	849
J. P. C.	73.7	169	1526	20.7	706
H. W. E.	73.0	168	1559	21.4	725

athletes with whom they were compared. Since in these comparisons we considered only individuals of similar height and weight, it is clear that these differences were not due to changes in body surface, but from what is known with regard to athletic training we may maintain that the increased katabolism is directly due to the larger proportion of active protoplasmic tissue in the body of the athletes, thus suggesting that the katabolism of the body is proportional not to the surface area, but to the mass of active protoplasmic tissue.

The detailed report of the investigation has been transmitted to the *Journal of Biological Chemistry*.

A COMPARISON OF THE BASAL METABOLISM OF NORMAL MEN AND WOMEN

By Francis G. Benedict and L. E. Emmes

NUTRITION LABORATORY, CARNEGIE INSTITUTION OF WASHINGTON

Presented to the Academy, January 13, 1915

From the earliest attempts to adjust food intake to the energy requirement it has been recognized that the dietetic needs of men as a class are somewhat greater than those of women. This increase has been commonly ascribed in large part to the variations in the muscular activity and yet there has been a definite belief that the basal energy requirement for women may be materially different from that for men. In connection with observations made on a large number of normal men and women, primarily for the purpose of comparing them with pathological subjects, we have accumulated the results of observations on 89 men and 68 women, all of whom were in 'presumably good health.' The experiments were made with essentially the same technique and with the subject in the same condition of muscular repose and the post-absorptive state, i.e., 12 hours after the last meal. Under these conditions, differences due to muscular activity are entirely eliminated and we obtain the basal normal caloric output of the individuals studied.

The total heat production, computed on the 24-hour basis, was greater with the men, i.e., 1638 calories as against 1355 calories with the women. Of special interest is the fact that the oxygen consumption per kilogram per minute—a unit of measurement commonly employed by physiologists—was remarkably uniform with both sexes, being 3.65 cc. with the men and 3.58 cc. with the women. On the basis of heat-production per 24 hours, the men showed 25.5 calories per kilogram of body weight and the women 24.9 calories. On the debatable unit of apportionment of heat-production per square meter of body surface the men had 832 calories per 24 hours against 772 calories for the women.

It should be borne in mind, however, that the 89 men had an average body-weight, without clothing, of 64.3 kg. and the 68 women a body-weight of but 54.5 kg.; furthermore, the average height of the men was 172 cm. and that of the women 162 cm. A direct, unqualified comparison of the total metabolism of these two classes, or of the metabolism per kilogram of body weight or per square meter of body surface is, therefore, not permissible for it is not logical to compare two groups of individuals with unlike weight and height.

From the groups of men and women it was possible to make selections of a number of individuals who had approximately the same height and weight; on this basis alone can an intelligent comparison be made. Eleven groups, with body weights varying from 67 kg. to 45 kg., were compared and it was found that the average heat-production of all eleven groups, computed on the basis of per kilogram of body weight, was 26.5 calories with men and 25.0 calories with women. The heat output of the men was greater in all but three of the eleven groups.

Using again the computation on the basis of the heat production per square meter of body surface we found that the average for the eleven groups showed 819 calories per 24 hours for the men as compared with 770 calories for the women. Of the eleven groups only two had lower values for the men than for the women.

On the basis of the foregoing paper, giving the results of a study of athletes, we rigorously excluded athletes from these comparisons, and hence we are dealing here with non-athletic men and women of the same height and the same weight. It is thus reasonable to suppose that the actual body surface of the different groups must have been very nearly the same, and it is not logical to assume that the larger heat production noted with the men was due to a disproportion between the body measurements and the body surface. We believe that these data show a basal metabolism for men some 5 or 6% greater than for women of similar height and weight, and that this increase is due to the fact that in all probability the women, particularly in those groups with the greater body weight, had a much larger proportion of subcutaneous fat than did the men, thus indicating a consequent smaller proportion of active protoplasmic tissue.

The detailed report of this investigation has been transmitted to the *Journal of Biological Chemistry*.

THE FACTORS AFFECTING NORMAL BASAL METABOLISM

By Francis G. Benedict

NUTRITION LABORATORY, CARNEGIE INSTITUTION OF WASHINGTON

Presented to the Academy, January 13, 1915

Although it is well known that the basal normal metabolism of individuals varies considerably, the exact cause for these variations is not known. For many years physiologists have believed that the heat production of a living body is proportional to the surface area. This most helpful hypothesis served to bring order out of chaos in the earlier

study of energy requirements and transformations in the living organism, but as experimental evidence accumulated, the validity of this so-called 'law of surface area' has been frequently called into question. An extended series of observations on 89 men and 68 women, all of them normal individuals in presumably good health, together with observations upon a group of athletes, a large number of new-born infants, normal infants under one year, and atrophic infants, and particularly a recent 31-day study of a fasting man, have given us data which warrant a reconsideration of the factors affecting basal metabolism.

When all of the observations on the normal men and women are plotted on charts, it is seen that there is no direct relationship between body weight and the total metabolism other than that, in general, people of large body weight have a greater metabolism than smaller individuals. However, there are so large a number of exceptions to this general rule that nothing approximating a physiological law can be derived from these observations. The same is true with regard to the computations of the heat production per kilogram of body weight, for the widest variations are found with our so-called normal people, with no tendency towards uniformity. Furthermore, it has been shown in a foregoing article that the total metabolism and the metabolism per kilogram of body weight are distinctly larger with athletes than with normal individuals. In all of these comparisons the differences between men and women have been recognized; hence separate plots for men and women have been made.

It has been practically impossible to make direct surface measurements of the men and women that we have studied, and we have relied, as have all physiologists hitherto, upon the commonly accepted formula of Meeh, in which the body surface is considered to be the cube root of the square of the weight multiplied by the constant 12.312. For infants the slightly smaller constant of 10.3, determined by Lissauer, has been accepted as the best available. Using Meeh's formula, we find that the metabolism of normal men, as computed from the body surface and expressed in calories per square meter of body surface per 24 hours, ranged from 693 to 958 calories. Since, according to the currently accepted belief in the constancy of heat production per square meter of body surface, we would expect to find constant values with all individuals, irrespective of size, it can be seen that this variation is extraordinarily large. It should furthermore be stated that both of these extremes were found with non-athletic individuals. With women the range was from 633 calories per square meter per 24 hours to 906 calories.

In neither of these comparisons is there any tendency to regularity or to a grouping of the plots. In other words, the evidence all points towards distinct individuality with no relationship between the computed body surface and the heat production.

With infants the variations in the metabolism on this basis are even greater. Thus it was found with normal and atrophic infants that the 24-hour heat production per square meter ranged from 554 calories to 1334 calories, while with the strictly normal infants the range was from 554 to 991 calories. It is obvious that any basis of comparisons which involves possible variations of 40% with men, 43% with women and 80% with normal infants cannot be considered as a physiological law.

On the other hand, an examination of the available material shows that there is a relationship between the heat production and the body composition, that is, that any physiological data which imply a difference in the proportion of active protoplasmic tissue are invariably accompanied by a difference in the basal metabolism. Thus the trained athletes showed a distinctly greater metabolism than did the non-athletic individuals. Furthermore, men with a smaller amount of subcutaneous fat and a correspondingly greater proportion of active protoplasmic tissue have been found to have a greater metabolism than women of the same height and weight. The study of normal and atrophic infants showed that with two infants of the same height and weight the elder, who would naturally be somewhat atrophic, invariably had a higher basal metabolism than the normal, well-nourished infant. Even with normal adults it can be maintained that of two individuals having the same weight but different heights, the taller individual will, in general, have the greater proportion of active protoplasmic tissue and the comparison of the heat production of normal men of like age and weight but different heights shows that there is almost invariably a distinctly greater metabolism with the taller individual. It should be remembered that in all of these comparisons only the basal metabolism, measured in complete muscular repose and in the post absorptive condition, is used.

A factor that has heretofore been neglected in considering basal metabolism is the possibility that the mass of active protoplasmic tissue may functionate with varying degrees of intensity. In comparing the metabolism of normal individuals we find that frequently individuals with approximately the same weight had very great variations in the heat production. This is true of a group of eight men weighing over 80 kgm. and likewise with a group of five men weighing 50 kgm. or under.

This, of itself, points strongly towards distinct differences in the intensity of cellular activity.

While the larger proportion of individuals studied were in youth, i.e., 20 to 30 years of age, a few were under 17 and a number over 40. On studying the plots showing the variations with normal individuals, both for men and women, a distinct tendency may be noted for the older people of both sexes to have a somewhat decreased metabolism. The evidence is equally as clear that in youth the metabolism is considerably increased, thus pointing towards an increased cellular activity in early youth and a decreased cellular activity or possible atrophy of active protoplasmic tissue with increasing age.

One of the most noticeable factors influencing metabolism is that of sleep. It has been commonly assumed that sleep *per se* does not affect metabolism, but it has been found in a long study of a fasting man that there were numerous metabolic planes during the day, showing that the stimulus to cellular activity must have varied considerably. With this fasting man neither the mass of protoplasmic tissue nor the surface area of the body could have altered materially in the course of 24 hours, yet we find that if we give a value of 100 to the basal metabolism during the night, when the subject was sound asleep, the value when the subject was lying awake in the morning, with complete muscular repose, would be 114, and in the late afternoon under the same conditions the value would be 122.

Our laboratory data include experiments with a considerable number of normal men covering several months, and in some cases, several years. A study of these data shows that the metabolism of 35 subjects on whom experiments were made 5 days or more apart and, on the average, several months apart, varied not far from 14%, although in all cases the subject was in complete muscular repose and in the post absorptive condition. Since during this period there was no material alteration in the body weight and consequently in the body surface, the variation in the metabolism must be ascribed to a difference in the stimulus to cellular activity.

In the fasting experiment not only was there a difference in the metabolic level noted at different times of the day due to the condition of being awake and asleep, and in the late afternoon after the experimental program of the day, but, as the 31-day fast progressed, the heat production per square meter of body surface varied from 859 calories on the first fasting day to 668 calories on the twenty-third fasting day. Although the loss in body weight was material, apparently the skin shrank

in proportion and there was no obvious disproportion between body weight and surface area. During the fourth week of the fast there was a distinct tendency for the metabolism to rise, accompanied by a measurable increase in the pulse rate. Thus the tendency for the prolonged fasting to depress the metabolism was actually overcome by some unknown stimulus increasing the cellular activity of the remaining body substance, ultimately resulting in a positive increase in the basal metabolism during the last week of fasting.

Two other factors have been experimentally demonstrated in this laboratory as influencing the metabolism. One of these is the character of the preceding diet, it having been shown that when a carbohydrate-free diet is eaten an acidosis is developed which distinctly increases the cellular activity and results in a very noticeable increase in the basal metabolism. The second factor is the marked after-effect of severe muscular work, such work causing the pulse rate and the basal metabolism to remain at an increased level for many hours after the cessation of the muscular activity. This may logically be ascribed to a stimulus to the protoplasmic tissue, resulting in a higher metabolic plane.

From the evidence cited, therefore, it may be concluded that there is no direct relationship between total body weight and total heat production, that the metabolism or heat output of the human body, even at rest, does not depend upon Newton's law of cooling, and is therefore independent of the surface area. The observations on athletes, the comparison of normal men and women, and of atrophic and normal infants indicate that the proportion of active protoplasmic tissue plays an important rôle. Perhaps the most striking factors causing variations in the stimulus to cellular activity are age, sleep, prolonged fasting, character of the diet, and the after-effect of severe muscular work.

From the evidence gathered with the various subjects studied it is clear that the basal metabolism of an individual is a function, first, of the total mass of active protoplasmic tissue, and, second, of the stimulus to cellular activity existing at the time the measurement of the metabolism was made.

The detailed report of this investigation has been transmitted to the *Journal of Biological Chemistry*.

ON THE NATURE OF THE NERVE IMPULSE

By Shiro Tashiro

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Read before the Academy, December 9, 1914. Received January 13, 1915

The nerve fiber, as is well known, shows not only the highest degree of irritability, but also possesses power to transmit a state of excitation in the most efficient manner. Our interest in the study of the nerve impulse lies not only in the fact that the nervous system is one of the most important factors in all organic activities in the higher animals, but also, in the fact that it constitutes the center of a general problem that deals with the mechanism by which a living substance adjusts itself to its environment.

In order to study the nature of the nerve impulse, many physiologists have investigated what takes place in the nerve fiber during the conduction of the impulse. The results were very peculiar: there were no structural changes of any kind, no heat production, and there appeared to be no production of carbon dioxide, nor did they find any change in the distribution of the organic materials in the tissue. When electrical changes were discovered in the stimulated nerve, the problem was considered to be settled once for all. The idea that the nerve impulse is of an electrical nature was, however, soon doubted, when Helmholtz observed that the velocity of the nerve impulse is of an entirely different magnitude from that of an electrical current. One thing seemed to be certain: that the nerve impulse can pass through the fiber without consuming any material. The fact that we can not detect fatigability in the nerve after continuous stimulation supported the belief that certain quickly reversible physical conditions exist in the nerve, and that changes in these conditions must determine the phenomena of excitability and conductivity in the nerve. Such changes were thought to be either of the colloidal state, of the surface tension, or of the permeability of the nerve.

With the new apparatus which measures as little carbon dioxide as one ten-millionth gram, we have demonstrated that an isolated nerve fiber gives off a definite amount of carbon dioxide and that when it is stimulated this carbon dioxide production is more than doubled. These facts not only indicate that there is another kind of functional change beside the electrical, but also show that the chemical changes in the nerve fiber are of the same magnitude as those of other organs.

To some physiologists, however, our results do not seem to be con-

clusive enough to show that the carbon dioxide given off by an isolated nerve is produced by life processes. Experimental conditions, to their minds, might have caused the tissue to decompose in a manner quite different from its normal behavior.

Further experiments show the following facts:

1. If the sciatic nerve is removed from a frog, it shows an electrical response for many hours. Since electrical changes are characteristic only of living nerves, we consider that the isolated nerve does not die for many hours. If measurements are made on an isolated nerve at successive time intervals, we find that carbon dioxide production steadily decreases as the nerve approaches death The point of minimum carbon dioxide production corresponds nearly to the point where electrical response ceases. The dead nerve gives almost no carbon dioxide.

2. Although the nerve remains active for some time without oxygen, it is a known fact that absence of oxygen diminishes the excitability of the nerve. The carbon dioxide production of a nerve fiber is much less in hydrogen than in atmospheric air. We should expect that there would be no difference under these two conditions if the carbon dioxide production of an isolated nerve fiber were due entirely to the death process.

3. The normal, uncut fiber in the body is very susceptible to many chemical reagents. A weak concentration of an anesthetic, for instance, renders the nerve nore excitable, or stimulates it, while a higher concentration makes it unexcitable. These reagents show similar effects on the carbon dioxide production of the cut nerve.

These facts, to my mind, indicate that the production of carbon dioxide from an isolated nerve fiber is at least a correct expression of what is going on in the normal nerve in the body, and indicate clearly that the normal nerve must have a chemical activity which is accelerated when the nerve is stimulated.

If the chemical activity is so vigorous as our results indicate, one naturally asks how we can explain the fact that the nerve impulse can pass continuously for hours without any measurable sign of fatigue. Apparent lack of fatigability in the nerve is a remarkable fact, but I wonder whether we should be willing to ignore the presence of metabolic activity in the contracting wings of insects which can fly continuously for hours at a rate of as high as three hundred vibrations per second. The ordinary induction coil we use for fatigue experiments, by the way, maintains not more than one hundred vibrations per second, at the maximum.

Granting that the absence of fatigability in the nerve, as measured by ordinary methods, may not be a question of absence of metabolism, but merely of the speed at which breaking and repairing processes of the tissue come to equilibrium, one may still ask how we explain the lack of heat production. Snyder reported very recently[1] that a smooth muscle failed to show any sign of heat formation during contraction, which, no one doubts, increases carbon dioxide production. When we dip a zinc rod into copper sulphate solution we observe heat formation, but that with proper arrangements, as in a Daniell cell, the reaction goes on practically isothermally, all the chemical energy being converted into electrical energy. I do not think that we should ignore the increase of carbon dioxide production in the nerve on stimulation because of the fact that we cannot detect heat formation.

There are some physiologists who admit that the living nerve should be chemically active to maintain a 'normal' condition, like any other living tissue, but who hold that the increase in carbon dioxide production on stimulation must be a secondary effect due to primary physical changes. We will consider this question in a quite different way.

The state of a nerve fiber depends upon three conditions: the degree of irritability, the direction of the impulse and the rate of the impulse. The relation of these conditions to chemical activity in the nerve is analyzed in the following way:

1. Degree of irritability and carbon dioxide production.

We have already cited the fact that chemical reagents which modify the degree of excitability invariably modify the rate of carbon dioxide production in the same proportion. The chemical activity in the nerve fiber seems to determine the state of nerve excitability.

2. The direction of nerve impulse and carbon dioxide production.

If one takes nerve bundles containing only sensory fibers, which conduct the normal nerve impulse in a central direction, the portion of the nerve nearer the natural source of the nerve impulse (i.e., nearer the end organ) gives more carbon dioxide than the portion away from it. There is a gradient of carbon dioxide production in the unstimulated nerve. This gradient of chemical condition seems to determine the direction of nerve impulse. Many experiments made on various kinds of pure nerve fibers enable us to generalize this by saying that the normal nerve impulse passes toward a point of lower carbon dioxide production.

3. Rate of the nerve impulse and carbon dioxide production.

There seems to exist a close relation between the rate of nerve impulse and carbon dioxide production in the resting nerve, if one com-

pares the corresponding nerves of different animals. The data for such a generalization must necessarily be cumulative. The limited data we have secured indicate that the nerves which give off more carbon dioxide in the resting state conduct the nerve impulse more quickly.*

I may add here that conditions which influence the speed of the nerve impulse modify the chemical activity of the resting nerve. It has been known for a long time that the temperature coefficient of velocity of the nerve impulse is greater than that of most purely physical processes. We find that the temperature coefficient of carbon dioxide production of the non-stimulated nerve is of about the same magnitude as that of the velocity of the nerve impulse.

Basing our conclusions on the foregoing experimental facts, we consider the nature of the nerve impulse as follows:

There are two chemical conditions necessary to enable the nerve to conduct the nerve impulse.

The first condition is the maintenance of normal chemical activity, i.e., the presence of certain chemically unstable substances. Just what these substances are, we do not know. This condition constitutes, to my mind, the so-called state of excitability. The nerve must be in this condition to be capable of response to a stimulus. The instability of these substances enables the nerve to undergo greater chemical changes when stimulated; that is, stimulation is accompanied by an increase in chemical activity. This is true not only for nerve tissue, but also for all other living tissues, including living seeds. The second condition is, if this increase in chemical change at the point of stimulus is sufficiently greater than that in its neighborhood, the impulse will go in that direction. The normal nerve impulse, therefore, will go only in one definite direction. The possibility of nerve conduction in two

* In this connection it is interesting to note that carbon dioxide production from medullated nerves is not always greater than from non-medullated ones, in spite of the fact that medullated nerves invariably carry the nerve impulse much more quickly than non medullated ones. This is rather important in view of the fact that the conducting mass in the medullated fiber is known to be much less than that in the non-medullated, if we compare the total nerve fibers gram for gram. It is very likely that if we could compare the carbon dioxide production from the reacting masses of the two nerve fibers, the relative rate of metabolism of the axis cylinder of the medullated fiber would be greater than that of the non-medullated. Since such a comparison is practically impossible at present, the only way to test the correctness of our hypothesis will be to make a series of carbon dioxide measurements on various medullated fibers and compare them with their corresponding rates of nerve impulse. If this relation proves to be general, then not only our current notion that the function of the medullary sheath is to supply nutrition to the conducting medium will be proved correct, but also we may easily understand the morphological development of the medullary sheath in relation to the functional activity of the nerve fiber.

directions is obvious. It must depend on the gradient of chemical activity along the fiber, and the amount of increase of this activity at the point of stimulation.

I believe the nerve impulse is a propagation of chemical change—the propagation being due to a restoration of an equilibrium disturbed by the increase of metabolism at the point of stimulus. This propagation is always toward the point where there is less chemical activity, as measured by carbon dioxide production.

[1] *Amer. J. Physiol.*, 35, 340.

A POINT SCALE FOR MEASURING MENTAL ABILITY

By Robert M. Yerkes

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Presented to the Academy, December 4, 1914

Alfred Binet, in 1905, devised a method of roughly estimating the intellectual capacity, or degree of mental development, of the child in terms of age. The method depended upon the application of series of single tests or measures, each series being especially arranged for a particular year of age. If a subject could satisfactorily meet the requirements for his age, he was considered up to the standard. Obviously he might measure either a certain number of years above or below the expected intellectual age.

The Binet measuring scale of intelligence has been revised, perfected, and adapted by various individuals, and now after nearly ten years of practical application, it stands as the only convenient and reasonably expeditious method of classifying children with respect to intelligence. It possesses, however, many serious defects which may not now be enumerated, since the purpose of this abstract is to present a brief description of a new method which is based, on the one hand upon the work of Binet and his associates, and on the other upon a suggestion made by the late E. B. Huey. We may call this new method the point scale for measuring mental capacity. It has been developed at the Psychopathic Hospital, Boston, as one result of the demand for reasonably detailed and reliable information concerning the mental characteristics of individuals both immature and mature. The scale consists of a single series of measurements to be made on all subjects. Each measurement is evaluated according to a graded scale, and the maximum credit obtainable in an examination is one hundred points. The

various measurements in question are distributed among the most important intellectual functions (affectivity being omitted), so that the several aspects—memory, imagination, perception, association, suggestion, judgment, ideation—may be measured.

The value of the point scale depends upon reliable norms. Two boys, from poor surroundings, each of the age seven years, six months, subjected to examination, obtain, for example, the one a credit of twenty-two points and the other a credit of fifty-nine points. By reference to our norms, we discover that boys of the age in question born to English-speaking parents of poor or medium circumstances, should obtain a score of 36—while those born to parents of good or superior circumstances should obtain 45. Since these norms, as is evident, take account of sex, age, language, and social status, it is clear that the first individual is nearly 40 per cent below our reasonable expectation in intellectual ability, whereas the second individual is instead about 37 per cent above what we should expect.

It thus is clear that instead of stating that an individual is a certain number of years and months above or below age, as in the Binet method, we can definitely state the relation of a given individual examined to a more or less inclusive group in which he belongs. This value we call the coefficient of mental ability. It is obtained by dividing the individual's score by the average or norm for his group. We may also, if it is desirable, state the exact frequency of occurrence in the group of the degree of intelligence indicated, as for example, one in one hundred, with respect to inferiority or superiority, as the case may be.

A single measure chosen from our scale must serve to indicate its chief features. We choose the test for the span of auditory memory. The subject is required to repeat, after the experimenter, groups of digits ranging in number from three to seven. These are presented by the experimenter orally, clearly and distinctly, at the rate of two per second. If the subject fails to repeat correctly the first group of a given number, he is given a second trial with another group containing the same number. If he succeeds with that, the experimenter passes on to a group containing one additional digit, and so on. One point credit is allowed for each of the five groups correctly reproduced. If a subject fails in both trials for a given number of digits, the observation is terminated. The test for memory-span is:

	1st trial	2nd trial	Credit
(a).....	374	581	(1)
(b).....	2947	6135	(1)
(c).....	35871	92736	(1)
(d).....	491572	516283	(1)
(e).....	2749385	6195847	(1)

The preliminary scale, which we have thoroughly tested by applying it to about eight hundred normal children and adults and to more than three hundred abnormal individuals, has proved so serviceable that we are now attempting to develop a more highly perfected and inclusive series of measurements which shall be universally applicable and shall take account of the affective as well as the intellectual functions.

Since the object of our work was the development of a practically serviceable method of measuring mental ability for use in hospitals, clinics, schools, reformatories, prisons, and wherever a rough estimate of mental status is demanded, it does not seem worth while to present results in this article.

The principles involved in the universal point scale are as follows:

1. A single series of measurements to be made on all subjects examined.

2. Gradation of each member or part of the scale with respect to difficulty so that measurement may be made, with equal facility, of the capacity of the child of three and the adult.

3. Partial credits according to the extent and nature of the response.

4. Distribution of the several measurements equally among the chief groups of mental processes: for example, according to the following four categories of processes, one-fourth of the measurements being devoted to the processes under each. (*a*) Sensibility, perceptivity, discrimination, association (receptivity); (*b*) Memory, in several of its aspects, and imagination (imagination); (*c*) Simple feeling, emotion, sentiment, volition, and suggestibility (affectivity); (*d*) Ideation, judgment, reasoning (thought).

5. Arrangement of the several measurements of the scale, probably twenty in all, in the four groups suggested above, namely, (*a*) receptivity; (*b*) imagination; (*c*) affectivity; (*d*) thought. So that, one-fourth of the scale being devoted to each group of processes, the credits achieved by an individual may conveniently be represented by a simple formula. Assuming that the maximum number of credits obtainable is two hundred and that individual *X* achieves one hundred and fifty-three points, his mental formula might be written thus:

$$X = \text{R}.43 + \text{I}.48 + \text{A}.22 + \text{T}.40 = 153.$$

Such a formula would indicate to the examiner that X is especially deficient or peculiar in affective characteristics.

6. The measuring scale shall be arranged on four pages, those measurements dealing with one of the four categories of mentality occupying a page. On each sheet, the several measurements shall be arranged in order of increasing difficulty, and the same shall hold of the order of arrangement within any given part of the series, that is, any one of the twenty types of measurement.

7. The measurements shall be chosen, so far as possible, with a view to simplicity of materials and ease and uniformity of observation.

8. The scale shall be dependent for its value upon safely determined norms.

COLOR VISION IN THE RING-DOVE (Tutur risorius)

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Presented to the Academy, December 4, 1914

The psychophysiological literature, both naturalistic and experiental, on color vision in infra-human animals, is surprisingly extensive. But even more surprising is the extreme uncriticalness of the methods which have been employed. A realization of this condition of affairs within the past decade led simultaneously to the development, by C. Hess, in Europe, and by R. M. Yerkes in association with J. B. Watson, and more recently by G. H. Parker, in America, of spectral methods for the comparative study of color vision. These methods enable the experimenter to measure and control his stimuli in their various aspects and to observe with reasonable accuracy organic response to specific stimuli. The method now in use in this country, developed by Watson and me, may be named from the nature of the stimulus and the form of reaction demanded 'the method of discriminating spectral stimuli.' It involves the use of a special form of prism spectrometer with devices for selecting, spacing, reversing, and displaying any two portions of the spectrum, with means of controlling the selected stimuli qualitatively and intensively, of measuring them in photometric and energy units, and of so presenting them to the reacting animal that it may, if capable of so doing, recognize them and react appropriately.[1]

Until very recently, it has been the prevalent opinion even among scientific persons that many, if not most of the vertebrates, possess fairly highly developed color vision, which in many instances is closely

similar to the human. That this opinion is erroneous and demands correction is proved by the results obtained by the use of spectral stimuli. It is now definitely known, for example, that, among the rodents, the mouse, rat, and rabbit well-nigh lack the ability to distinguish colors. For them, long wave-lengths are of surprisingly low stimulating value. The spectral range of certain day-birds appears to be similar to that of man. Among the higher animals, critical observation indicates that the cat and dog possess slight power to respond differentially to different wave-lengths. The observations at present available do not justify dogmatic statements, but they conclusively prove that all earlier accounts of color vision in these animals are misleading.

During the past ten years, I have devoted a large amount of time to the development of methods and the accumulation of facts. My study of vision in the dancing mouse,[2] which went far toward proving the absence of color discrimination in that organism, led me to abandon the use of stimuli obtained from colored papers, cloths, or the use of ray-filters, and to depend wholly upon spectral light.

At present I have undertaken the study of color vision in the ring-dove,[3] using the Watson-Yerkes spectral light apparatus with a type of reaction-box which was developed in connection with the study of the dancing mouse. The method which is employed in connection with this apparatus has been called 'the discrimination method.' Watson has recently suggested the name 'sensory habit method.' It involves the simultaneous presentation to an organism of two stimuli which differ definitely and measurably in some respect or respects. It demands of the animal that it react differently to the two stimuli; to the one of them positively, to the other negatively. Positive reaction is encouraged by the reward of food and inappropriate response is discouraged by disagreeable electrical stimulation. The essential features of the apparatus are, first, a home compartment in which the animal receives stimulation from two adjacent sources. If, in a certain required manner, it approaches the stimulus which has been defined as the 'positive,' it is permitted to escape thence by way of a narrow passage to a food compartment where it receives its reward. If, on the contrary, it approaches the stimulus designated as the 'negative,' it is punished by electrical stimulation, is forced to retreat, and then again has the opportunity to react to the 'positive' stimulus.

The sensory habit apparatus, even in its most improved form, is a fairly simple mechanism, and the sensory habit method, with the employment of reward and punishment as conditions for careful discrimi-

nation and habit formation, has proved remarkably satisfactory and has yielded valuable results in numerous investigations.

It has thus far been demonstrated that the ring-dove may be trained, although rather laboriously, to appropriate forms of achromatic discrimination, and that similarly it may be used by the sensory habit method for studies of chromatic discrimination. Experiments with two individuals demonstrate the existence of the Purkinje phenomenon in this organism. This was done by use of a red of 630 $\mu\mu$ and green of 505 $\mu\mu$, approximately equal in energy. In general illumination, the two stimuli seemed to be of approximately equal stimulating value for the female, whereas for the male, the red was the more stimulating. A moderate degree of adaptation to darkness reversed this relation, the green then being much more stimulating than the red for the female, whereas the two were approximately equal for the male.

This observation indicates at once changing sensitiveness with adaptation and an individual, or more likely, a sex, difference in the stimulating value of the colors in question. Apparently the male ring-dove is markedly more sensitive to red than is the female.

This report of work which is still in progress may be concluded with a word concerning the significance of temperamental differences. In the doves in question, these were very striking indeed and are responsible for certain important features of the results obtained. The female was tame and timid; the male, on the contrary, somewhat wild and bold. In view of these facts, it is not surprising that the male proved an excellent subject for the sensory habit method, whereas the female was much less satisfactory. The suggestion from the work is that temperament should be analyzed by the student of animal behavior and its various important components separately studied and accurately measured so that our animal subjects may be described with respect to the same, even although the investigation especially in point be one on color vision or some other aspect of reaction or experience which might at first seem unrelated to 'temperament.'

[1] See Yerkes, Robert M. and Watson, John B. Methods of studying vision in animals. *Behavior Monographs*, vol. 1, no. 2. (1911).

[2] Robert M. Yerkes. *The dancing mouse.* New York, 1907.

[3] This work was made possible by a grant from the Bache Fund of the National Academy, for which the writer takes this opportunity to express his hearty thanks.

THE FEEBLY INHIBITED, II. NOMADISM OR THE WANDERING IMPULSE, WITH SPECIAL REFERENCE TO HEREDITY

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Presented to the Academy, January 12, 1915

Some persons are always satisfied to remain at home and dislike the thought of traveling; at another extreme are the tramps and nomadic gypsies; at still another extreme are those who, capable of steady and effective work, periodically, often in a more or less dazed condition, run away from their homes. The term nomadism is here adopted for this trait in all of its varied manifestations, the racial connotation of the term being advantageous rather than otherwise.

Nomadism has been widely studied by psychiatrists who have seen in the various periodic disorders with which it is often associated the causes of the different forms that it takes. Thus are distinguished demented, melancholic, epileptic, hysteric and other "fuges." The present study starts with the inquiry: Are the similar symptoms that are associated with such different mental states wholly independent, or have they a common cause?

A tendency to wander in some degree belongs to all locomotor animals. That such a tendency is a fundamental instinct in man also is indicated by four lines of evidence: (1) that the anthropoid apes (representing the primate stock from which man sprang) are nomads; (2) that primitive peoples (Fugeians, Australians, Bushmen, Hottentots) are nomads and this trait is widespread among other, less primitive, tribes; (3) that the tendency to wander is nearly universal among young children who have only recently learned to walk; (4) and that, at adolescence, the instinct (no doubt associated with the mating impulses) becomes keen again.

To get further light on nomadism one hundred family histories, deposited at the Eugenics Record Office, have been analyzed and tabulated according to the nature of the matings. The results of this tabulation is given on the following page.

This table shows that far more males than females are nomadic, 171 males to 15 females. This suggests the hypothesis that nomadism is a sex-linked trait. If this hypothesis be true, in a mating of a normal man and a woman who, though normal, comes of nomadic stock, half of the sons and none of the daughters are nomadic. Combining matings 1, 6, 7, and 8, which are the matings in question, we get the following

distribution of tendencies in the offspring. Males: W, 133; not W, 109; females: W, 4; not W, 118. The shortage of not-W males is easily accounted for by the circumstances that only fraternities of offspring showing nomadics are included in our table; any fraternity which by chance, through small size or otherwise, shows no nomads is excluded. Of the four female nomads (where none is expected) it is to be said that all are daughters of three men who are really quite insufficiently known and ought not to be considered in this connection. The details are set forth in the full paper. There is no clear case of a nomadic daughter whose father is known to be non-nomadic.

TABLE OF DISTRIBUTION OF NOMADISM (W) IN OFFSPRING OF VARIOUS MATINGS

PARENTS	OFFSPRING						
	TOTAL	x	†yg	♂		♀	
				W	NOT W	W	NOT W
1. Father not W, of not-W strain; Mother not W, of W strain	208	13	29	71	43	1	52
2. Father not W, of not-W strain; Mother, W	9	1	1	5±	(3±)	0	0
3. Father and Mother both W	13	0	2	6	1	4	0
4. Father, W; Mother not W, of W strain	35	6	7	8	5	2	7
5. Father W; Mother not W, W relatives unknown	73	17	6	16	16	5	13
6. Father not W, of W strain; Mother not W, of W strain	3	0	0	1	0	0	2
7. Father not W, of W strain; Mother not W, W relatives unknown	55	8	6	13	16	1	11
8. Relatives of father and mother (and frequently the parents themselves) little known	218	33	28	51	47	2	53

Abbreviations: W, wandering impulse, nomadism; x, unknown; † yg, died young.

The criterion that all sons of a nomadic mother are nomadic is tested by reference to matings 2 and 3. All but four sons are nomadic. Of the exceptions three are recorded with great doubt and one with much lack of detail.

The next criterion, that all daughters of two nomadic parents are nomadic is realized, in mating 3, unfortunately in too small numbers.

The criterion that half of the daughters and half of the sons of nomad-bearing fraternities derived from nomadic fathers are nomadic may be tested in matings 4 and 5. The expected equality is, indeed, found in the sons but, probably on account of small numbers, inexactly in the

daughters. In general our Histories show that nomadic fathers may have no nomadic sons, but there is no case of a nomadic mother of more than two children none of whom is nomadic. Thus our data support the hypothesis that the paralysis of the control of the momadic impulse is a sex-linked trait.

Other hypotheses, such as (1) that nomadism is, like the beard in man, essentially a male characteristic, and (2) that nomadism is less common among women than among men solely because it is less feasible for women to live a nomadic life, are not at all supported by a full consideration of the facts.

Finally, the frequent association of nomadic impulses with psychoses both periodic (depressions, migraine, hysteria, sprees) and constitutional (feeblemindedness and dementia-precox) is explained as follows. The wandering instinct is, in man, fundamental, basic. In the more intellectual part of the population, under the influence of the *mores*, this impulse is more or less satisfactorily repressed where there is good reason why it should be, except in families of *periodics* where the inhibitions are temporarily paralyzed and the person 'breaks out.' Among constitutional mental inferiors on the other hand, the inhibitions may be slightly developed and such persons show a constant roving tendency—as ne'er-do-wells, some tramps, the gypsies and other nomadic tribes. All classes of nomadism can thus be ascribed to one fundamental cause, the nomadic instinct. In addition, since the more intelligent representatives of the nomadic race are able, in a measure, under ordinary circumstances, where it appears desirable, to inhibit their impulses, we find that, with such people, the unstable, wandering impulse is apt to be associated with a periodic disturbance that renders inoperative the inhibitory machinery; these disturbances are not the fundamental cause of the nomadic impulse but merely permit it to show itself. The capacity for such periodic disturbance is sex-linked.

The complete paper will probably appear in the *Journal of Nervous and Mental Disease* and in the *Bulletin of the Eugenics Record Office.*

ANNOUNCEMENTS

WILLIAM ELLERY HALE LECTURES

The third course of lectures on the William Ellery Hale Foundation will be given by Professor Thomas Chrowder Chamberlin, of the University of Chicago, at the next annual meeting of the Academy, on "The Evolution of the Earth."